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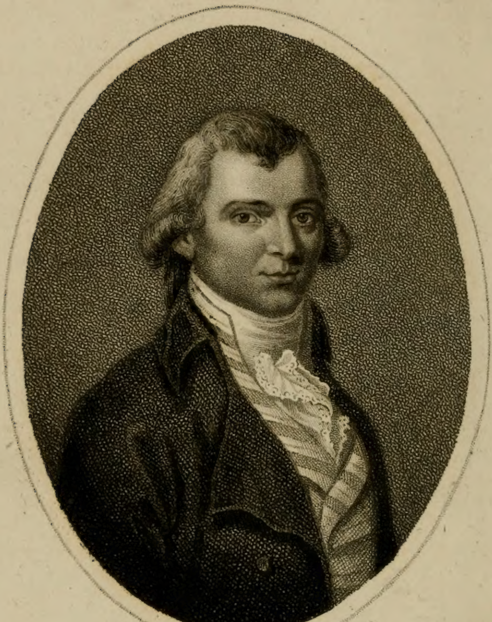

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Dr Olbers

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THE

PHILOSOPHICAL MAGAZINE:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

AGRICULTURE, MANUFACTURES,

AND

COMMERCE.

BY ALEXANDER TILLOCH,

HONORARY MEMBER OF THE ROYAL IRISH ACADEMY, &c. &c. &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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THE

THE
PHILOSOPHICAL MAGAZINE.

I. *The Bakerian Lecture, on some Chemical Agencies of Electricity.* *By* HUMPHRY DAVY, *Esq. F. R. S. M. R. I. A.**

1. *Introduction.*

The chemical effects produced by electricity have been for some time objects of philosophical attention; but the novelty of the phænomena, their want of analogy to known facts, and the apparent discordance of some of the results, have involved the inquiry in much obscurity.

An attempt to elucidate the subject will not, I hope, be considered by the society as unfitted to the design of the Bakerian lecture. I shall have to detail some minute, and I fear tedious, experiments; but they were absolutely essential to the investigation. I shall likewise, however, be able to offer some illustrations of appearances which hitherto have not been fully explained, and to point out some new properties of one of the most powerful and general of material agents.

II. *On the Changes produced by Electricity in Water.*

The appearance of acid and alkaline matter in water acted on by a current of electricity at the opposite electrified metallic surfaces, was observed in the first chemical experiments made with the column of Volta †.

Mr. Cruickshank ‡ supposed that the acid was the nitrous acid and the alkali ammonia. M. Desormes § soon after

* From the *Transactions of the Royal Society*, part i. for 1807.

† Nicholson's Journal, 4to., vol. iv. p. 183.

‡ Ibid. vol. iv. p. 261.

§ Annales de Chimie, tom. xxxvii. p. 233.

attempted to show by experiments that muriatic acid and ammonia were the products; and M. Brugnatelli * asserted the formation of a new and peculiar substance, which he has thought proper to call the electric acid. The experiments said to be made in Italy, and in this country, on the production of muriate of soda are recent †, and the discussions with regard to them still alive. As early as 1800, I had found, that when separate portions of distilled water, filling two glass tubes connected by moist bladders, or any moist animal or vegetable substances, were submitted to the electrical action of the pile of Volta by means of gold wires, a nitro-muriatic solution of gold appeared in the tube containing the positive wire, or the wire transmitting the electricity, and a solution of soda in the opposite tube ‡: but I soon ascertained that the muriatic acid owed its appearance to the animal or vegetable matters employed; for when the same fibres of cotton were made use of in successive experiments, and washed after every process in a weak solution of nitric acid, the water in the apparatus containing them, though acted on for a great length of time with a very strong power, at last produced no effect upon solution of nitrate of silver.

In cases when I had procured much soda, the glass at its point of contact with the wire seemed considerably corroded; and I was confirmed in my idea of referring the production of the alkali principally to this source, by finding that no fixed saline matter could be obtained by electrifying distilled water in a single agate cup from two points of platina connected with the Voltaic battery. Similar conclusions with regard to the appearance of the muriatic acid had been formed by the Galvanic Society of Paris by Dr. Wollaston, who hit upon the happy expedient of connecting the tubes together by well washed asbestus; and by Messrs. Biot and Thenard §.

* Phil. Mag. vol. ix. p. 181.

† By M. Pacchiani and by Mr. Peele, Phil. Mag. vol. xxi. p. 279.

‡ I showed the results of the experiment to Dr. Beddoes at this time; and mentioned the circumstance to sir James Hall, Mr. Clayfield, and other friends in 1801.

§ No. xl. *Du Moniteur,* 1806.

Mr.

Mr. Sylvester, however, in a paper published in Mr. Nicholson's journal for last August, states, that though no fixed alkali or muriatic acid appears when a single vessel is employed, yet that they are both formed when two vessels are used. And to do away all objections with regard to vegetable substances or glass, he conducted his process in a vessel made of baked tobacco-pipe clay inserted in a crucible of platina. I have no doubt of the correctness of his results; but the conclusion appears objectionable. He conceives that he obtained fixed alkali, because the fluid, after being heated and evaporated, left a matter that tinged turmeric brown, which would have happened had it been lime, a substance that exists in considerable quantities in all pipe clay; and even allowing the presence of fixed alkali, the materials employed for the manufacture of tobacco-pipes are not at all such as to exclude the combinations of this substance.

I resumed the inquiry; I procured small cylindrical cups of agate, of the capacity of about 1-4th of a cubic inch each. They were boiled for some hours in distilled water, and a piece of very white and transparent amianthus that had been treated in the same way, was made to connect them together; they were filled with distilled water, and exposed, by means of two platina wires, to a current of electricity from 150 pairs of plates of copper and zinc four inches square, made active by means of solution of alum. After 48 hours the process was examined: paper tinged with litmus plunged into the tube containing the transmitting or positive wire, was immediately strongly reddened. Paper coloured by turmeric introduced into the other tube had its colour much deepened; the acid matter gave a very slight degree of turbidness to solution of nitrate of silver. The fluid that affected turmeric retained this property after being strongly boiled, and it appeared more vivid as the quantity became reduced by evaporation; carbonate of ammonia was mixed with it, and the whole dried and exposed to a strong heat: a minute quantity of white matter remained, which, as far as my examination could go, had the properties of carbonate of soda. I compared it with similar minute portions of the pure carbonates of potash and soda. It was not so deli-

quescent as the former of these bodies, and it formed a salt with nitric acid, which, like nitrate of soda, soon attracted moisture from a damp atmosphere, and became fluid.

This result was unexpected, but it was far from convincing me that the substances which I had obtained were generated. In a similar process, with glass tubes, carried on exactly under the same circumstances, and for the same time, I obtained a quantity of alkali which must have been more than twenty times greater, but no traces of muriatic acid. There was much probability that the agate might contain some minute portion of saline matter, not easily detected by chemical analysis, either in combination or intimate adhesion in its pores. To determine this, I repeated the experiment a second, a third, and a fourth time. In the second experiment turbidness was still produced by solution of nitrate of silver in the tube containing the acid, but it was less distinct; in the third process it was barely perceptible; and in the fourth the two fluids remained perfectly clear after the mixture. The quantity of alkaline matter diminished in every operation; and in the last process, though the battery had been kept in great activity for three days, the fluid possessed in a very slight degree only the power of acting on paper tinged with turmeric; but its alkaline property was very sensible to litmus paper slightly reddened, which is a much more delicate test; and after evaporation and the process by carbonate of ammonia, a barely perceptible quantity of fixed alkali was still left. The acid matter in the other tube was abundant; its taste was sour; it smelt like water over which large quantities of nitrous gas have been long kept; it did not affect solution of muriate of barytes; and a drop of it placed upon a polished plate of silver left after evaporation a black stain, precisely similar to that produced by extremely diluted nitrous acid.

After these results I could no longer doubt that some saline matter existing in the agate tubes had been the source of the acid matter capable of precipitating nitrate of silver, and of much of the alkali. Four additional repetitions of the process, however, convinced me that there was likewise some other cause for the presence of this last substance; for it

it continued to appear to the last in quantities sufficiently distinguishable, and apparently equal in every case. I had used every precaution: I had included the tubes in glass vessels out of the reach of the circulating air; all the acting materials had been repeatedly washed with distilled water; and no part of them in contact with the fluid had been touched by the fingers.

The only substance which I could now conceive capable of furnishing the fixed alkali was the water itself. This water appeared pure by the tests of nitrate of silver and muriate of barytes; but potash and soda, as is well known, rise in small quantities in rapid distillations; and the New River water, which I made use of, contains animal and vegetable impurities, which it was easy to conceive might furnish neutral salts capable of being carried over in vivid ebullition.

To make the experiment in as refined a form as possible, I procured two hollow cones of pure gold containing about 25 grains of water each; they were filled with distilled water, connected by a moistened piece of amianthus which had been used in the former experiments, and exposed to the action of a Voltaic battery of 100 pairs of plates of copper and zinc of six inches square, in which the fluid was a solution of alum and diluted sulphuric acid. In ten minutes the water in the negative tube had gained the power of giving a slight blue tint to litmus paper; and the water in the positive tube rendered it red. The process was continued for 14 hours; the acid increased in quantity during the whole time, and the water became at last very sour to the taste. The alkaline properties of the fluid in the other tube, on the contrary, remained stationary, and at the end of the time it did not act upon litmus or turmeric paper more than in the first trial: the effect was less vivid after it had been strongly heated for a minute; but evaporation and the usual process proved that some fixed alkali was present. The acid, as far as its properties were examined, agreed with pure nitrous acid having an excess of nitrous gas.

I repeated the experiment, and carried on the process for three days; at the end of which time the water in the tube

was decomposed and evaporated to more than one-half of its original quantity; the acid was strong, but the alkali in as minute a portion as in the last experiment. It acted, indeed, rather more vividly on the tests, on account of the greater diminution of the fluid, but presented the same results after being heated.

It was now impossible to doubt that the water contained some substance in very minute quantities, capable of causing the appearance of fixed alkali, but which was soon exhausted; and the question that immediately presented itself was, Is this substance saline matter carried over in distillation? or is it nitrogen gas which exists in minute portions in all water that has been exposed to air, and which, if an element of the fixed alkali, would under the circumstance of the experiment have been soon exhausted, whilst its absorption from the atmosphere would be impeded by the saturation of the water with hydrogen?

I was much more inclined to the former than to the latter supposition. I evaporated a quart of the distilled water that I had used, very slowly, at a heat below 140° Fahrenheit, in a silver still; a solid matter remained, equal to 7-10ths of a grain; this matter had a saline but metallic taste, and was deliquescent when exposed to air: I could not obtain from it regular crystals; it did not affect turmeric or litmus, but a part of it, after being heated red, in a silver crucible, exhibited strong alkaline properties. It was not possible to make a minute analysis of so small a quantity, but it appeared to me to be principally a mixture of nitrate of soda and nitrate of lead; and the metallic substance, it is most likely, was furnished by the condensing tube of the common still.

The existence of saline matter in the distilled water being thus distinct, it was easy to determine its operation in the experiment. I filled the two gold cones with water in the usual manner; that negatively electrified, soon attained the maximum of its effect upon turmeric paper. I then introduced into it a very minute portion of the substance obtained by the process of evaporation that has been just described:

scribed: in less than two minutes its effects were evident; and in five minutes the tint of the paper was changed to a bright brown.

I now conceived that, by collecting the water obtained in the second process of slow distillation, I should be able to carry on the experiment without any appearance of fixed alkali; and the trial proved that I was not mistaken.

Some of this water was introduced into the gold tubes, and the amianthus moistened by it.

After two hours the water in the negative tube produced no effect upon turmeric paper; it did produce an effect upon litmus, which it required great minuteness of observation to perceive; but it wholly lost the power by being heated strongly for two or three minutes, so there is every reason for supposing that it was owing to a small quantity of ammonia.

I made a similar experiment with a portion of the same water in the tubes of agate that had been so often used, and I had the pleasure of finding the results precisely the same.

To detail any more operations of this kind will be unnecessary; all the facts prove that the fixed alkali is not generated, but evolved, either from the solid materials employed, or from saline matter in the water.

I have made many experiments in vessels composed of different substances, with the water procured by very slow distillation; and in almost every instance some fixed alkali appeared.

In tubes of wax the alkaline matter was a mixture of soda and potash; and the acid matter a mixture of sulphuric, muriatic, and nitric acids.

In a tube of resin the alkaline matter seemed to be principally potash.

A cube of Carrara marble of about an inch, having an aperture in its centre, was placed in a crucible of platina, which was filled as high as the upper surface of the cube with the purified water; the aperture was filled with the same fluid: the crucible was positively electrified by a strong Voltaic power, and a negatively electrified wire introduced into the aperture.

The

The water soon gained the property of affecting the tint of turmeric; and fixed alkali and lime were both obtained from it: and this effect took place in repeated experiments: the fixed alkali, however, diminished in quantity every time; and after eleven processes, conducted from two to three hours each, disappeared altogether. The production of lime water was uniform.

I made a solution of 500 grains of this marble in nitric acid; I decomposed the mixture by carbonate of ammonia, and I collected and evaporated the fluid part, and decomposed the nitrate of ammonia by heat. About 3-4ths of a grain of fixed saline matter remained, which had soda for its base.

It was possible that the Carrara marble might have been recently exposed to sea water; I therefore tried, in the same way, a piece of granular marble, which I had myself broken from a rock on one of the highest of the primitive mountains of Donegal. It afforded fixed alkali by the agency of negative electricity.

A piece of argillaceous schist from Cornwall, treated in the same manner, gave the same result; and serpentine from the Lizard, and grauwackè from North Wales, both afforded soda. It is probable that there are few stones that do not contain some minute portions of saline matter, which in many cases may be mechanically diffused through their substance; and it is not difficult to conceive the possibility of this, when we consider that all our common rocks and strata bear evident marks of having been antiently covered by the sea.

I was now able to determine distinctly that the soda procured in glass tubes came principally from the glass, as I had always supposed.

I used the two cones of gold with the purified water and the amianthus; the process was conducted as usual. After a quarter of an hour, the negatively electrified tube did not change the colour of turmeric. I introduced into the top of it a bit of glass; in a few minutes the fluid at the surface rendered the tint of the paper of a deep bright brown.

I had never made any experiments in which acid matter having

having the properties of nitrous acid was not produced, and the longer the operation the greater was the quantity that appeared.

Volatile alkali likewise seemed to be always formed in very minute portions, during the first few minutes, in the purified water in the gold cones, but the limit to its quantity was soon attained.

It was natural to account for both these appearances from the combination of nascent oxygen and hydrogen respectively, with the nitrogen of the common air dissolved in the water; and Dr. Priestley's experiments on the absorption of gases by water (on this idea) would furnish an easy explanation of the causes of the constant production of the acid, and the limited production of the alkali: for hydrogen, during its solution in water, seems to expel nitrogen; whilst nitrogen and oxygen are capable of co-existing dissolved in that fluid *.

To render the investigation more complete, I introduced the two cones of gold with purified water under the receiver of an air pump: the receiver was exhausted till it contained only 1-64th of the original quantity of air; and then, by means of a convenient apparatus, the tubes were connected with an active Voltaic pile of fifty pairs of plates of four inches square. The process was carried on for 18 hours, when the result was examined. The water in the negative tube produced no effect upon prepared litmus, but that in the positive tube gave it a barely perceptible tinge of red.

An incomparably greater quantity of acid would have been formed in a similar time in the atmosphere, and the small portion of nitrogen gas remaining in contact with the water seemed adequate to the effect.

I repeated the experiment under more conclusive circumstances. I arranged the apparatus as before; I exhausted the receiver, and filled it with hydrogen gas from a convenient airholder; I made a second exhaustion, and again introduced hydrogen that had been carefully prepared. The process was conducted for 24 hours, and at the end of this

* Priestley's Experiments and Observations, vol. i. p. 59.

time

time neither of the portions of the water altered in the slightest degree the tint of litmus.

It seems evident then, that water, chemically pure, is decomposed by electricity into gaseous matter alone, into oxygen and hydrogen.

The cause of its decomposition, and of the other decompositions which have been mentioned, will be hereafter discussed.

III. *On the Agencies of Electricity in the Decomposition of various Compounds.*

The experiments that have been detailed on the production of alkali from glass, and on the decomposition of various saline compounds contained in animal and vegetable substances, offered some curious objects of inquiry.

It was evident that in all changes in which acid and alkaline matter had been present, the acid matter collected in the water round the positively electrified metallic surface, and the alkaline matter round the negatively electrified metallic surface; and this principle of action appeared immediately related to one of the first phænomena observed in the Voltaic pile—the decomposition of the muriate of soda attached to the pasteboard; and to many facts which have been since observed on the separation of the constituent parts of neutrosaline and metallic solutions, particularly those detailed by Messrs. Hisinger and Berzelius *.

The first experiments that I made immediately with respect to this subject were on the decomposition of solid bodies, insoluble, or difficultly soluble in water. From the effects of the electrical agency on glass, I expected that various earthy compounds would undergo change under similar circumstances; and the results of the trials were decided and satisfactory.

Two cups made of compact sulphate of lime, containing about 14 grain measures of water each, were connected together by fibrous sulphate of lime, which was moistened by pure water: the cups were filled with this fluid; platina wires from the Voltaic battery of 100 pairs of plates of six

* *Annales de Chimie*, tom. li. p. 167.

inches

inches were introduced into them, so that the circuit of electricity was through the fibrous sulphate of lime. In five minutes the water in the cup connected with the positive wire became acid; that in the opposite cup strongly tinged turmeric. After an hour the fluids were accurately examined; when it was found that a pure and saturated solution of lime had been produced in the cup containing the negative wire, which was partially covered with a crust of lime, and that the other cup was filled with a moderately strong solution of sulphuric acid.

I procured two cubical pieces of crystallized sulphate of strontites of about an inch; a hole was drilled in each capable of containing about eight grains of water: the cubes were plunged in pure water in a platina crucible, and the level of the fluid preserved a few lines below the surface of the cubes; two platina wires were introduced into the holes, which were filled with pure water. The disengagement of gas, when the wires were connected with the battery of 100, proved that the sulphate of strontites was sufficiently porous to form a proper conducting chain. The results were much longer in being obtained in this experiment than in the last; some time elapsed before a sensible effect could be perceived, but the termination was similar. In thirty hours the fluid in the cavity containing the negative wire had gained the property of precipitating solution of sulphate of potash, and the presence of sulphuric acid in the other cavity was evident from its effect upon solution of muriate of barytes.

I made an experiment upon fluate of lime under like circumstances; but the crystallized fluate, not being equally permeable to moisture, the two cavities were connected by moist asbestus. This decomposition was likewise very slow; but in the course of two days a pretty strong solution of lime was obtained in one tube, and an acid fluid in the other, which precipitated acetite of lead, and left a spot upon the glass from which it had been evaporated.

Sulphate of barytes, as might be supposed, proved much more difficult of decomposition than either sulphate of strontites or fluate of lime. I had made four or five experiments upon it, with the same kind of apparatus that had been applied

plied to the fluate of lime, before I was able to gain decided results. In the last process performed on this substance, two pieces of a large single crystal were hollowed by grinding, so as to contain about five grains of water each; they were connected by moist asbestus, and constantly subjected during four days to the strong action of a battery of 150 pairs of plates of four inches square. As the water diminished, its place was supplied by new quantities. At the conclusion of the experiment the fluid on the positive side of the apparatus instantly reddened litmus, tasted very sour, and gave a distinct precipitate with a solution of muriate of barytes; the water on the other side deepened the tincture of turmeric, but did not render solution of sulphate of potash turbid. There was a small quantity of white crust, however, on the sides and the bottom of the cavity, and I conceived that this might be the barytes, which, during the extremely slow decomposition, would have combined with the carbonic acid of the atmosphere. To ascertain if this had been the case, I introduced into the cavity a drop of diluted muriatic acid; a slight effervescence appeared, and the fluid obtained occasioned a distinct white cloudiness in solution of sulphate of soda.

In all these cases the constituent parts of the bodies newly arranged by the effects of electricity existed in considerable quantities, and exposed on a large surface to its action. I had great reason to believe, however, from the trials with distilled water in different vessels, that very minute portions of acid and alkaline matter might be disengaged by this agency from solid combinations, principally consisting of the pure earths.

This part of the investigation was easily elucidated.

For a purpose of geological inquiry, which on a future occasion I shall have the honour of laying before the society, I had made a careful analysis of a specimen of fine grained basalt from Port Rush, in the county of Antrim, by means of fusion with boracic acid: it had afforded in 100 parts 3½ parts of soda, and nearly one-half a part of muriatic acid, with 15 parts of lime. This stone appeared to me very well fitted for the purpose of experiment: cavities were drilled

drilled in two pieces, properly shaped; they contained about twelve gains of water each; they were connected by moistened amianthus, and the process conducted, as usual, with a power of fifty pairs of plates. At the end of ten hours the result was examined with care. The fluid that had been positively electrified had the strong smell of oxymuriatic acid, and copiously precipitated nitrate of silver; the other portion of fluid affected turmeric, and left by evaporation a substance which seemed to be a mixture of lime and soda.

A part of a specimen of compact zeolite, from the Giant's Causeway, which by analysis had given seven parts in 100 of soda, had a small cavity made in it; it was immerged in pure water in a crucible of platina, and electrified in the same manner as the cube of Carrara marble, mentioned in page 9. In less than two minutes the water in the cavity had gained the property of changing the colour of turmeric, and in half an hour the solution was disagreeably alkaline to the taste. The matter dissolved proved to be soda and lime.

Lepidolite, treated in the same way, gave potash.

A piece of vitreous lava, from Ætna, gave alkaline matter, which seemed to be a mixture of soda, potash, and lime.

As in these trials the object was merely to ascertain the general fact of decomposition, the process was never conducted for a sufficient time to develop a quantity of alkaline matter capable of being conveniently weighed, and of course any loss of weight of the substance could not be determined.

I thought it right, however, to make one experiment of this kind, for the sake of removing every possibility of doubt on the source of the different products; and I selected for this purpose glass, as a substance apparently insoluble in water, and not likely to afford in any way erroneous results.

The balance that I employed was made for the Royal Institution, by Mr. Fidler, after the model of that belonging to the Royal Society: it turns readily with $\frac{1}{500}$ of a grain when loaded with 100 grains on each side; a glass tube with a platina wire attached, weighing 84 grains $\frac{56}{1000}$, was connected with an agate cup by amianthus; they were filled with purified water, and electrified by a power from 150 pairs

pairs of plates, in such a way that the platina in the glass tube was negative. The process was continued for four days, when the water was found alkaline. It gave by evaporation and exposure to a heat of about 400° Fahrenheit, soda mixed with a white powder insoluble in acids, the whole weight of which was $\frac{36}{128}$ of a grain. The glass tube carefully cleaned and dried weighed 84 grains $\frac{37}{128}$. The difference between the loss of weight of the tube and the weight of the products in the water may be easily explained; some minute detached particles of amianthus were present, and the soda must have contained water, a substance which it is probably perfectly free from in glass.

Having obtained such results with regard to the disengagement of the saline parts of bodies insoluble in water, I made a number of experiments on soluble compounds; their decomposition was always much more rapid, and the phænomena perfectly distinct.

In these processes I employed the agate cups with platina wires, connected by amianthus moistened in pure water; the solutions were introduced into the cups, and the electrifying power applied from batteries of fifty pairs of plates, in the usual way.

A diluted solution of sulphate of potash treated in this manner, produced in four hours at the negative wire a weak lixivium of potash, and a solution of sulphuric acid at the positive wire.

The phænomena were similar when sulphate of soda, nitrate of potash, nitrate of barytes, sulphate of ammonia, phosphate of soda, succinate oxalate, and benzoate of ammonia, and alum, were used. The acids in a certain time collected in the tube containing the positive wire, and the alkalies and earths in that containing the negative wire.

Solutions of the muriatic salts, decomposed in the same way, uniformly gave oxymuriatic acid on the positive side.

When compatible mixtures of neutrosaline solutions containing the common mineral acid were used, the different acids and the different bases seemed to separate together in a mixed state, without any respect to the orders of affinity.

When metallic solutions were employed, metallic crystals

or

or depositions were formed, as in common galvanic experiments, on the negative wire, and oxide was likewise deposited round it; and a great excess of acid was soon found in the opposite cup. With solutions of iron, zinc, and tin, this effect took place, as well as with the more oxidable metals: when muriate of iron was used, the black substance deposited upon the wire was magnetic, and dissolved with effervescence in muriatic acid; and when sulphate of zinc was used, a gray powder possessed of the metallic lustre, and likewise soluble with effervescence, appeared; and in all cases acid in excess was exhibited on the positive side.

Strong or saturated saline solutions, as might have been expected, afforded indications of the progress of decomposition much more rapidly than weak ones; but the smallest proportion of neutrosaline matter seemed to be acted on with energy.

A very simple experiment demonstrates this last principle. If a piece of paper tinged with turmeric is plunged into pure water in a proper circuit in contact with the negative point, the very minute quantity of saline compound contained in the paper affords alkaline matter sufficient to give it instantly a brown tint near its point of contact; and acid in the same manner is immediately developed from litmus paper at the positive surface.

I made several experiments with the view of ascertaining whether in the decompositions by electricity the separation of the constituent parts was complete from the last portions of the compound; and whenever the results were distinct, this evidently appeared to be the case.

I shall describe one of the most conclusive of the experiments:—A very weak solution of sulphate of potash, containing twenty parts of water and one part saturated solution, at 64°, was electrified in the two agate cups by the power of fifty pairs of plates for three days: the connecting amianthus, which had been moistened with pure water, was removed, washed with pure water, and again applied, twice every day; by this precaution the presence of any neutral salt that might adhere to it, and disturb the results, was prevented. The alkali obtained in this process in the solution

tion had the properties of pure potash; and when it had been saturated with nitric acid it gave no turbidness by mixture with solution of muriate of barytes: the acid matter exposed to a strong heat evaporated without leaving any residuum.

[To be continued.]

II. *On the Hindoo Formulæ for computing Eclipses, Tables of Sines, and various Astronomical Problems. By M.* DELAMBRE*.

THESE formulæ may be found in the second volume of the Asiatic Researches, or Memoirs of the Society established at Calcutta; the two first volumes of which have lately been translated into French, and published in 1805 from the Imperial printing office.

Although these formulæ must have been known in Europe for some time, yet as the original memoirs, printed first at Calcutta, and since reprinted in London, are very rare in France, we thought it our duty to announce them to our readers, many of whom, most likely, have not heard of them.

Ducham, Bailly, and Le Gentil, have mentioned that the Indians possess methods of computing eclipses, which they follow without understanding. Mr. Davis, the English author of this memoir, has attacked this assertion victoriously, by giving at full length the computation of the eclipse of the moon of November 1789, by means of the Indian formulæ, of which he has given the demonstration and explanation according to the *Souria Siddanta*. Want of room obliges us to suppress these details; observing merely, that we have revised the computations with attention, and that, excepting a few points of the Indian doctrine, and a few suppositions whose foundations are not very intelligible, we can declare that all the rest are as clear as the composition of the subject will admit:

* From the *Connoissance des Temps* for 1808. Translated by T. S. Evans, F. L. S.—The ingenious method contained in this paper for computing *Sines*, and explained by M. Delambre, is curious, and will prove interesting to our astronomical readers.

admit: but I cannot forbear saying a few words on the Indian tables of sines, and of the two methods by which they have been computed; for, since they have been printed, I have found that in the note put at the bottom of this table I have not sufficiently appreciated the excellence of the method, from having been drawn into an error by a constant number, which does not appear to have been given accurately enough in the memoir.

In this table the sines are expressed in minutes; they proceed by intervals of $3\frac{3}{4}$ degrees, and suppose the radius 3438, or rather 3437·75. On the side of the right sines it gives the versed sines.

If we examine the process recommended by the Indian author, we readily perceive that his method is reduced to the computing beforehand a first difference, which is at the same time the first sine of the table; after which, to obtain the second sine, he calculates the second difference, which he subtracts from the first difference; this gives him the first difference between the first and second sine, and then the second sine: after this he computes another second difference, to obtain a new first difference and a new sine, and so on to the end of the table. This process is exactly that which I have pointed out in the preface to Borda's Decimal Tables, without knowing that the method which appeared unknown even to the moderns, had been practised so long ago in India.

My* formula is $\Delta^{(2)} s,A = -4\,s^2,\frac{1}{2}\,\Delta A s,A = (-\text{chord})^2$ $\Delta\,A\,s,A$. (See page 48 of the Decimal Tables.)

Δ A being a constant quantity in a table of sines, it follows, that to obtain the second difference of any sine whatever, it is necessary to multiply this sine by a constant number: now Δ A in the Indian tables is $3^\circ\ 45'$, therefore $4\,s^2,$ $\frac{1}{2}\,\Delta\,A = 4\,s^2,\ 1^\circ\ 52'\ 30'' = 0{\cdot}0042821 = \frac{1}{233{\cdot}53}$; whence it follows, that the constant factor to find the second difference is $\frac{1}{233{\cdot}53}$; that is to say, the last found sine must be

* Delambre uses Δ to denote *first difference*, and $\Delta^{(2)}$ to denote *second difference*.

divided by $\frac{1}{233 \cdot 53}$: but according to the memoir this constant divisor is $\frac{1}{225}$, which makes me think there is an error of the press; and the more so, as the Indian author's numbers do not agree well with this divisor 225, but, instead of which, with my divisor $\frac{1}{233 \cdot 53}$; and following the precept literally, I find the Indian numbers very nearly as we may perceive.

		Indian Sines.	Sines by my Divisor.	First Differences.	Sec Differ
0°	0′	000	000·00		
3	45	225	224·85	224·85	
7	30	449	448·75	223·89	0·96
11	15	671	670·71	221·97	1·92
15	0	890	889·81	219·10	2·87
18	45	1105	1105·10	215·29	3·81
22	30	1315*	1315·56	210·56	4·73
26	15	1520*	1520·59	204·93	5·63
30	0	1719	1719·01	198·42	6·51
33	45	1910	1910·07	191·06	7·36
37	30	2093	2092·95	182·88	8·18
41	15	2267	2266·85	173·92	8·96
45	0	2431	2431·08	164·21	9·71
48	45	2585	2584·88	153·80	10·41
52	30	2728	2727·61	142·73	11·07
56	15	2859	2858·66	131·05	11·68
60	0	2978*	2977·47	118·81	12·24
63	45	3084	3083·55	106·06	12·75
67	30	3177*	3176·30	92·86	13·20
71	15	3256	3255·54	79·25	13·61
75	0	3321	3320·95	65·31	13·94
78	45	3372	3372·04	51·09	14·22
82	30	3409	3408·59	36·65	14·44
86	15	3431	3430·74	22·05	14·60
90	0	3438	3438·10	+7·36	14·69
93	45		3430·74	−7·36	14·72

This table supposes the radius greater than 3437·7, and less than 3438·4; according to Archimedes the radius would be between 3436·3 and 3438·5; mean 3437·4.

It

It is evident that, excepting a few sines, in which we differ some tenths, the agreement is perfect throughout the table; but with the divisor 225 we obtain only the three first sines with any accuracy, after which the error increases very rapidly. I suppose this false divisor is only a repetition of the divisor 225, which served to find the first of the first differences.

The Indian author does not show how he found his divisor; we can therefore only examine it by the work: now the work shows that a divisor differing very little from mine must be used.

This process is extremely curious; nothing like it is to be found in Ptolemy's Trigonometry, and all the authors on that subject were turned over to no purpose before we could find any vestige of it, till we came to Briggs, who was acquainted with this divisor, which he appeared to have discovered by the work, after comparing the second differences found out by other methods. Briggs himself did not know that it was the square of the chord of the differential arc Δ A.

But it may be asked why the Indians have made $\Delta A = 3^\circ 45'$ instead of 1°. The following, I think, is the reason; it appears to carry great probability:—It cannot be doubted that the Indians were acquainted with the following theorems: $s^2{,}A + c^2{,}A = \text{rad.}^2$; ver. $\sin{,}A = \text{rad.} - \cos{,}A = 2s^2{,}\tfrac{1}{2}A$; whence $s{,}\tfrac{1}{2}A = (\tfrac{1}{2}\text{ rad.} - \tfrac{1}{2}c{,}A)^{\frac{1}{2}}$. Now these three theorems are sufficient to find all the sines of their table, and will not give any other: they have therefore done all that they were able to do, and their table shows us the bounds of their knowledge: we see, indeed, p. 290, that they have actually used these three formulæ to compute their table, and that they knew, besides, that $s{,}30^\circ = \tfrac{1}{2}$ rad., which appears to leave not a doubt respecting what I have said. Their table being constructed, they then examined the first and second differences, and remarked that the first constantly decreased, but they could not immediately perceive according to what law; the second differences, on the contrary, continually increased, and it was not difficult to perceive that they were

 proportional

proportional to the sine: thus the second difference opposite 30° is 7·36, and that opposite 90° is 14·72, double of the former: and to find the ratio of the second difference to the sine they divided the radius 3437·75 by 14·72, and they found 233·53 for their quotient: dividing thus each sine by its second difference they constantly found this same quotient, whence they concluded that to obtain this second difference it was only necessary to divide the sine by 233·53.

The rule for the first differences is not so simple; for the difference of $s, A = 2s, \frac{1}{2} A c, (A + \frac{1}{2} \Delta A)$, and the $s, (A + \frac{1}{2} \Delta A)$, are not in the table.

But the first of the first differences is at the same time the first sine of the table; whence they concluded, that with the first sine, and the first of the first and second differences, they were fully prepared for computing all the rest: but in the work the table was already computed throughout, when it gave them their differential method; and the proof of this is, that to make their table as they have given it they had occasion to make the first sine 224·85 and not 225, which would have given the first differences a little too great, and the sines too small.

It is true the *Souria Siddanta* recommends to divide the number of minutes in a sign by 8 to obtain the first sine, which is the same as making the sine equal to the arc; thus $\frac{30°}{8} = \frac{360°}{96} = \frac{21600'}{96} = 225' = 3° 45'$; instead of which the true value found by the above theorems is only 224·85.

Let it be observed that there is nothing conjectural but the reasoning which I have given them, for they really possessed all the knowledge which I suppose them to have had. I do not pretend, however, that they used decimal fractions; it is only to shorten the work that I have used them in reconstructing their table of sines, for it is well known all their calculations were made in sexagesimals.

By taking proportional parts, the use of which was well known to them, they might have extended their table to every degree; but these interpolated degrees would have had their *signs* less accurate, and they have preferred giving those which result immediately from their formulæ, to preserve in all

all its purity the table which was to serve for computing all the others: but they have given their tables of the equation of the centre for every degree.

Their theory for computing these tables of equations was incomplete and inaccurate; although they used epicycles, like the Greeks, to compute the inequalities of the planets, this calculation was with them less geometric than those of Ptolemy. They had introduced an empiric correction, which was very badly conceived; and they supposed that from 90° to 180° the same equations returned in an inverted order. The Greeks, in this respect, were further advanced than the Hindoos; their trigonometry was much more complete, although that of the Hindoos had a greater resemblance to ours; and the Hindoos appear to have had some theorems unknown to the Greeks. These tables of equations, although defective, are, nevertheless, very curious: in the explanation of them given by the Hindoos we observe that the differences of the equation are proportional to the sine of the anomaly; or, which is nearly the same, that the variation of the sine is proportional to the cosine.

It is evident also from this memoir, that the Hindoos found the latitude of a place by computing the length of the shadow of a gnomon, especially when the sun was in the equator; they might have found it also by the solstitial shadow, using the sun's greatest declination, which according to them was 24°.

To determine their longitudes they observed eclipses, and compared them with the computations made from their lunar tables adapted to their first meridian.

In page 315 is shown their method of calculating the sun's right ascension by means of their sines, without knowing the tangents.

In the same place is also shown how they computed the ascensional differences and the point of the equator which rises with each sign. The table which they made has been published by M. Le Gentil, who acknowledged he was unacquainted with the principle on which it was constructed: this principle is given in the memoir, and I have explained it at full length in a note.

We shall not enter into any discussion respecting the antiquity of the *Souria Siddanta**: by only considering the form of their tables, their ideas of the precession of the equinoxes, their obliquity of 24°, and their theory of eclipses, we believe the authors of the Hindoo astronomical books more antient than the astronomers of Alexandria. On the other hand, when we find things known to them that were unknown to the Greeks, we should be inclined to consider them of a more recent date. The system of epicycles for the planets is common to them both, but less perfect than that of the Greeks; whence we may conjecture that this doctrine of the Indians passed from them to Greece, where it was extended and improved. It appears less natural to suppose that the Hindoos received them from the Greeks through the intermedium of the Arabs, on account of the imperfect and disfigured theories which are found among them. We can, however, affirm that this memoir, although nothing is to be obtained from it that can improve our present knowledge or the progress of astronomy, is nevertheless extremely curious, and worthy the attention of astronomers. What renders the reading of it more difficult is, the great number of Indian technical words contained in the translation. They ought to have given a second version, in which there should have been no other than their own language: I had that idea, but to execute it completely some notions were necessary which I could not obtain, and some researches to which I had not time to attend.

In the same memoir we observe that the great period of the Indians, which is 4,320,000 years, is only an astronomical period, at the beginning of which the mean longitudes, the aphelia, and the nodes of all the planets were nothing. At the end of this memoir we find another tending to prove that the Indian zodiac has not been borrowed either from the Greeks or the Arabs. We find also two memoirs on Indian chronology by sir William Jones.

In the appendix, p. 68, is given a memoir by Reuben Bur-

* An ingenious Englishman made this book 3840 years old; but since that, in 1799, he has reduced this number to 731, or about the year 1268 of our æra.

rows,

rows, where he wishes to prove that Newton's binomial theorem was known to the Indians. In the notes I have answered the proofs which he brings forward to support his opinion, as they appear to me to have no foundation.

The first volume, which is very interesting to orientalists, contains nothing for astronomers; but every reader will see there, with great pleasure, a memoir on the gods of Italy, Greece, and India, by sir William Jones, with notes by M. Langlès.

III. *Problems on the Reduction of Angles. By* T. S. Evans, *F.L.S., of the Royal Military Academy, Woolwich* *.

The following problems are concerning the reduction of angles from one point or plane to another. They are not all of them new. One or two, with their solutions, will be found among the writings of each of the following celebrated mathematicians; Boscovich, Cagnoli, Carnot, Delambre, and probably some others: but few, if any of them, have ever been published by our English authors. It was therefore thought, if their solutions were given in one uniform manner, in our own language, they might be of service; and this is what I have here attempted, with a few additions of my own.

As they bear a very near relation to both plane and spheric trigonometry, but cannot, with propriety, be considered as actually belonging to either of them separately, I have taken the liberty of classing them under the title of *Goniometry*.

Their utility will be evident to every person who has any concern in geodesic operations. It seldom happens in practice that all the three objects at the angular points of a triangle are situated precisely in the horizontal plane passing through the observer's eye; therefore when one or two of them are above or below that plane the angles are different, and will require to be reduced to what they would be if the three

* Communicated by the Author.

three objects were exactly on the same level: otherwise the positions, if put down in a map, would be erroneous, as the distances between them would be too great. This reduction is easily effected by the assistance of goniometry.

When an instrument similar to colonel Mudge's is made use of, having both an altitude and azimuth circle, it is clear all the angles, being referred to one horizon, require no further reduction than what is necessary to be allowed on account of the earth's sphericity. But if, instead of an instrument of this kind, a sextant be used, and some of the objects be elevated or depressed, then the angles, not being all of them referred to the horizon, will require the aid of our theorems to reduce them to this plane.

Before we enter upon our subject we shall premise the following particulars:

In the first place, all geodesic angles of elevation are affected by the horizontal refraction; it is therefore necessary to show how the quantity of it may be found.

To determine the horizontal Refraction.

Rays of light which pass through the atmosphere, and which are neither reflected nor absorbed by it, do not proceed in a straight line, but are continually bent towards the earth as they go on; for they change their direction in passing obliquely from one medium to another of a different density; as for example, when they pass from air to water, or from glass to air. The atmosphere being composed of an infinity of layers, whose densities increase as they approach the earth, the rays of light, as they pass through, undergo the same change as if they had passed successively through an infinity of different mediums: they must therefore be inflected towards the earth in proportion as the density increases. But as the density of the air at different altitudes does not alter suddenly, but slowly, or by insensible degrees, the rays of light do not describe a polygon in passing through the atmosphere, but a regular curve line that is concave towards the earth's surface. The effect of refraction is therefore to make objects appear higher above the horizon than what they really are.

Hence

Hence it is evident that the point B (fig. 1.) viewed at A will appear elevated to D, and that A seen from B will be elevated to E. Let two observers, at the same moment, take the zenith distances of these two objects. Let the zenith distance of B, observed at A, be, on account of the refraction, ZAD; and that of A from B, zBE: now, supposing A and B to be at equal distances from C, then by drawing the line AG through A parallel to EF, it is evident, since the angle GAC = ACF,

that BAC = $90° - \frac{1}{2}$ ACB, and

DAC = 180 − ZAD;

therefore taking their difference BAD = $90° + \frac{1}{2}$ ACB − ZAD; whence ZAB = $90° + \frac{1}{2}$ C, or the true zenith distance of one object seen from the other is equal to 90° plus half the contained terrestrial arc. Therefore by comparing the observed angles with this when the earth's radius and the distance of the objects are known, the effect of the horizontal refraction will be had.

When one of the places is above the earth's surface, as at E, then, by nearly the same mode of reasoning, we find ZEB + zBE = 180° + C: but in this case AE and BE are easily obtained, which will give the angles CEB and ABE, and consequently ZEB and zBE; whence by comparison, as before mentioned, the horizontal refraction is obtained.

The usual method of allowing for the horizontal refraction is to take it as some fractional part of the terrestrial arc, or angle formed at the earth's centre by the distance between the object and the observer. Thus a distance of forty miles subtends an angle at the earth's centre of about 17′ 20″; consequently, the zenith distance of an object distant forty miles would be about 90° 8′ 40″, or it would be depressed 8′ 40″ below the horizon: now suppose he had found this angle by his instrument to be 90° 7′ 26″, the difference between this apparent and the true angle is 1′ 14″, which is about the $\frac{1}{14}$th part of 17′ 20″, and from this instance we should take the horizontal refraction at about $\frac{1}{14}$th of the contained terrestrial arc.

After a number of observations of the quantity of this horizontal

horizontal refraction have been made, and compared with the terrestrial arc, a sufficiently accurate rule may be deduced for correcting the observations in all cases, without finding how much it amounts to in every instance. Colonel Mudge has taken it at $\frac{1}{12}$th of the contained arc (Trigonometrical Survey, part i. p. 173); and as the instruments which this ingenious gentleman made use of were of the best kind and used with the utmost care, there seems to be greater reason to depend upon his results than upon those of any other person.

In some geodesic operations it happens that we have *to resolve a triangle which has one angle very obtuse or very acute.* Let ABC (fig. 2.) be an oblique angled triangle, whose vertical angle A is very obtuse; and having given the two sides together with this angle, let it be required to determine the base BC.

Continue BA to D, and draw CD perpendicular to BD; then $BC^2 = BD^2 + CD^2$, but $BD^2 = BA^2 + AD^2 + 2\,AB.\,AD$ therefore $BC^2 = AB^2 + AD^2 + 2\,AB.\,AD + CD^2$: and since $AD^2 + CD^2 = AC^2 \therefore BC^2 = AB^2 + AC^2 + 2\,AB \times AD$. But rad. : AC : : c, DAC or BAC : AD = AC × c, BAC, which being substituted for its equal, we get $BC^2 = AB^2 + AC^2 + 2\,AB.\,AC.\,c,BAC$. But (Cagnoli, 154.) $c,BAC = 1 - \frac{BAC^2}{2} + \frac{BAC^4}{2.\,3.\,4} - \&c.$: taking the first two terms, which are sufficient for our purpose, and putting them for their equals, we have $BC^2 = AB^2 + AC^2 + 2\,AB,\ AC\ (1 - \frac{BAC^2}{2}) = AB^2 + AC^2 + 2\,AB.\,AC - AB \times AC.\,BAC^2 = (AB + AC)^2 - AB.\,AC.\,BAC^2$: extracting the square root on both sides of the equation, and neglecting the powers of BAC above the square, we get $BC = AB + AC - \frac{AB.\,AC.\,BAC^2}{2\,(AB + AC)}$; but as the angle BAC is commonly given in minutes and seconds, we must divide it by R′ or R″; therefore, taking the latter, putting S = the sum of the sides, and P their product,

$$BC = S - \frac{P}{S} \times \frac{BAC^2}{2\,R''\,R''}.$$

Which

Which formula will be found extremely useful on a variety of occasions.

Let there now be given (see fig. 3.) *two sides and the included angle to determine the third side, when the given angle is very acute.*

By the same mode of investigation we get here $BC^2 = AB^2 + AC^2 - 2\,AB.\,AC.\,c, BAC$; and Cagn. 154, $c, BAC = 1 - \frac{BAC^2}{2}$ nearly; whence $BC^2 = AB^2 + AC^2 - 2\,AB.\,AC.\,(1 - \frac{BAC^2}{2}) = AB^2 + AC^2 - 2\,AB.\,AC. + AB \times AC.\,BAC^2 = (AB - AC)^2 + AB.\,AC.\,BAC^2$, and extracting the root of both sides $BC = AB - AC + \frac{AB.\,AC.BAC^2}{2\,(AB - AC)}$; dividing BAC by R″ for the reason before given, putting D = diff. of sides, and P = product, we have

$$BC = D + \frac{P}{2\,D} \times \frac{BAC^2}{R''\,R''}.$$

From these two formulæ we easily derive others for finding the angle in either of the two cases when the *three sides* are given. Thus the angle being very *obtuse*,

$$BAC'' = R'' \sqrt{\frac{2\,S}{P}\,(S - BC)};$$

and when the angle is very *acute*,

$$BAC'' = R'' \sqrt{\frac{2\,D}{P}\,(BC - D)}.$$

In carrying on a series of triangles it is usual to select such objects for the angular points as are most distinctly to be seen from each other on account of their elevation; and among them it frequently happens that the pointed spire of a steeple, or the flagstaff on the top of a tower, is chosen. This object, perhaps, can be observed extremely well from the other two angular points of the triangle, and the spire or flagstaff bisected by the vertical wire of the telescope with the greatest accuracy. But as it is desirable to measure all three of the angles of each triangle, we often find, to our great mortification, that when the instrument is removed to this third object it cannot be so placed as to have its centre immediately under or over the centre of the flagstaff or spire that

that was bisected; and even if it could, the walls of the building would prevent the angle from being measured. The three following problems show the method of obtaining this angle when the instrument, for the reasons just given, has necessarily been placed on one side of this centre.

Problem I.

In measuring the angles of the triangle ABC (fig. 4.), suppose the angular point A to be in the centre of a steeple, where the instrument cannot be placed to observe the quantity of the angle BAC.

Let D be a convenient place situated on the line AC for fixing the instrument, when on account of some impediment it cannot be used at A. Then let the angles CBA, CBD be observed at B, and let the angle CDB be observed at D: these will be sufficient to determine the three angles of the triangle ABC.

For $CBA - CBD = DBA$, and $CAB = CDB - DBA$; whence, as A and CBA are known, ACB is known also.

If D were the place of the steeple, and A the station fixed upon, then $CDB = CAB + ABD$, which reduces the angle A to that at D. But it seldom happens that the object C can be seen from A through the steeple at D; therefore in general it is necessary to choose the station D within the triangle ABC, or on one side of A.

If the angle DBA cannot be observed from B, some practical method must be had recourse to for determining it. Thus,

If Dp be let fall perpendicular to AB, then BD and Dp being known, the correction DBA is readily obtained; for $\frac{Dp}{DB \times ,1''} = \frac{Dp \times R''}{DB}$ = the number of seconds in the angle DBA. The side DB need not be known with extreme accuracy for this purpose.

In other cases, the following general equation may lead to a solution:

$$\overline{\dot{C}D} : \left.\begin{matrix} s,\dot{B} \\ -\ s,\dot{D} \end{matrix}\right\} :: BD : s,(D - \dot{D}) :: BD + \overline{\dot{B}D} : s,D.$$

in which it will be easy to substitute the value of any quantity

tity that is not known, in terms of other quantities that are known.

The Theorem, p. 29, will sometimes find its application in this reduction.

If two stations, D and d, be chosen any where in the lines AC, AB, and the angles C*Dd* and B*d*D be observed, then $180° - BdD = AdD$ and $180° - CDd = ADd$, but $180° - (ADd + AdD) = BdD - CDd - 180° = BAC$.

If the station d be so chosen that the angle C*d*B may be equal to the angle CDB, then the three angles of the triangle ABC may be obtained without actually measuring either of them. For, when $CDB = CdB$ a circle will pass through the four points C*Dd*B and $BCD + BdD = 180° = CDd + CBd$; therefore $BCD = 180° - BdD$, also $CBd = 180° - CDd$ and $A = CDd + BdD - 180°$; whence the three angles A, B, C, are obtained by only measuring the two C*Dd* and B*d*D.

Problem II.

When the point D is not situated in either of the lines AC or AB, but within the triangle.

In this case (fig. 5.) we have $BDE - DBA = BAE$, and $CDE - DCA = EAC$; consequently, $BDE + CDE - (DBA + DCA) = BAE + EAC$; whence we obtain the angle $BAC = BDC - (DBA + DCA)$.

If the steeple were at D and the station at A, then the angle $BDC = BAC + DBA + DCA$.

Problem III.

When the station D is without the triangle on one side of A (fig. 6).

In this case, $CFB = FAB + FBA = CDF + FCD$; therefore $FAB = CDF + FCD - FBA$; whence we get the angle $CAB = CDB + ACD - DBA$.

If A be situated on the other side of D (fig. 7), then, by the same mode of proceeding, the angle $CAB = CDB + ABD - DCA$.

When the small angles ACD, ABD cannot be measured from the stations B and C, some simple method must be resorted to for finding them as before observed.

The

The following is one method of reducing the angle BDC to BAC when the side AB and the angle CBA are known; supposing that the side BP does not differ much from AB, and that we know the length of AD.

Describe a circle to pass through the three points A B C. (Fig. 8.) Then CAB = CPB, but BPC = BDC + DBP, and PB : PD : : s, BDP : s, PBD $= \frac{s,\ \mathrm{BDP.\ PD}}{\mathrm{PB}}$. Multiplying this by R″, or dividing it by the sine of one second, we obtain the number of seconds in the angle PBD $= \frac{s,\ \mathrm{BDP.\ PD}}{\mathrm{PB.}\ s,\ 1''}$; consequently, the angle CPB = CAB = CDB + DBP = CDB + $\frac{s,\ \mathrm{BDP.\ PD}}{\mathrm{PB.}\ s,\ 1''}$. But in the triangle APD we have s, APD : s, DAP : : AD : DP, whence DP $= \frac{\mathrm{AD}\ s,\ \mathrm{DAP}}{s,\ \mathrm{APD}}$ and APC = DAP + ADP, consequently DAP = APC − CDA; therefore by substitution PD $= \frac{\mathrm{AD.}\ s,\ (\mathrm{APC}-\mathrm{CDA})}{s,\ \mathrm{APD}}$. But APC = ABC standing on the same arc AC, therefore PD $= \frac{\mathrm{AD}\ s,\ (\mathrm{ABC}-\mathrm{CDA})}{s,\ \mathrm{APD}}$; hence we get CAB = CPB =

$$\mathrm{CDB} + \frac{s,\ \mathrm{BDP.\ PD}}{\mathrm{PB.}\ s,\ 1''} = \mathrm{CDB} + \frac{\mathrm{AD}s,(\mathrm{ABC}-\mathrm{CDA})s,\mathrm{BDP}}{s,\ \mathrm{APD.}\ s,\ 1''.\ \mathrm{PB}}:$$

or, since PB = AB nearly, and APD = 180° − CBA, and s, APD = s, CBA; therefore CAB = CDB + $\frac{\mathrm{AD}\ s,(\mathrm{ABC}-\mathrm{CDA})\ s,\mathrm{BDP}}{s,\ \mathrm{ABC.\ AB.}\ s,\ 1''}$.

Or it may be resolved thus, when the distance AD is known, and either the actual lengths of the two sides AB, AC, or the ratio of AD to each of them:

First, BAC = BFC − ACD = BDC + ABD − ACD, but s, ABD $= \frac{\mathrm{AD}\ s,\ \mathrm{BDA}}{\mathrm{AB}} = \frac{\mathrm{AD}\ s,\ (\mathrm{BDC}+\mathrm{ADC})}{\mathrm{AB}}$ and s, ACD $= \frac{\mathrm{AD}\ s,\ \mathrm{CDA}}{\mathrm{AC}}$; therefore the number of seconds in ABD will be $= \frac{\mathrm{AD}\ s,\ (\mathrm{BDC}+\mathrm{ADC})}{\mathrm{AB}\ s,\ 1''}$, and those in ACD $= \frac{\mathrm{AD}\ s,\ \mathrm{CDA}}{\mathrm{AC}\ s,\ 1''}$; hence, by substituting these values for their

equals

equals in the first equation, we have $CAB = BDC + \frac{AD\,s, (BDC + ADC)}{AB\,s, 1''} - \frac{AD\,s, CDA}{AC\,s, 1''}$.

We might here extend our inquiry to the investigation of expressions for those particular cases where it is necessary to consider the forms of the bases of steeples or other buildings, the situation of their sides with respect to the sides of the triangles that are measuring, and the distance of the instrument from the walls of the building; but the variety is so very numerous, that only a comparatively small number could possibly be given here: it was therefore thought best to leave this to the observer himself, who will adapt his solution to the particular case that may occur.

Problem IV.*

Let ABD, ABS, and SBD (fig. 9.), be three planes perpendicular to each other, so that the three angles at B may be right angles; it is required to find an equation expressing the relation subsisting between the angles ASB, BSD, and ASD.

If we consider SB as radius, we shall have $BD = SB\,t$, BSD; $BA = SB\,t$, ASB; $SD = SB\,\int$, BSD; $SA = SB\,\int$, ASB; and $AD^2 = AB^2 + BD^2 = SB^2\,t^2$, $ASB + SB^2\,t^2$, BSD; but by trigonometry $c, ASD = \frac{AS^2 + SD^2 - AD^2}{2\,AS, AD} = \frac{\int^2, ASB + \int^2, BSD - t^2, ASB - t^2, BSD}{2\int, ASB\,\int, BSD}$; and since $\int^2, - t^2$

* This problem may be resolved by spherics in the following manner:

Let SBDA (fig. 10.) be the same figure as fig. 9, having BD perpendicular to the plane SBA; and suppose Z to be the zenith of the station S, then the arc ZC will be the complement of the angle of elevation DSB, and the arc CE will measure the oblique angle DSA, also the arc ZE will be 90°; hence we have the three sides ZC, CE, EZ, to find the quantity of the angle CZE, which it is evident will be equal to the angle BSA; for it is proved by the writers on spherics, that the spheric angle CZE is equal to the angle formed by the two tangents intersecting at Z; but these two tangents would be parallel to the lines AS, BS, intersecting at S, therefore the angle CZE is equal to the angle BSA. The triangle in this instance being rectilateral, we may consult Cagnoli's Trigonometry, p. 254, art. 440; or Maskelyne's Introd. to Taylor's Log. p. 42.

 $= 1$,

$= 1$, and $\frac{r^2}{f} =$ cos.; therefore the theorem, by reducing, becomes $\qquad c, \text{ASD} = c, \text{ASB}\, c, \text{BSD},$
the equation required; from which any two of the angles being given, the third may be found.

Hence, when we have given the angle formed at S by two objects A and D, one of which, A, is in the horizon, and the other, D, elevated above it, together with the angle of elevation of D, we may easily find the horizontal angle formed by these objects; for,

$$c, \text{ASB} = \frac{c, \text{ASD}}{c, \text{BSD}} = c, \text{ASD}\, f, \text{BSD}.$$

Problem V.

Let it be required to find the relation between the angles ASB, CSD, ASC, and BSD (fig. 11.), when all the angles at B are right, and the figure AD a parallelogram.

Taking SB as radius, we have by trigonometry $\text{SA} = \frac{\text{SB}}{c, \text{ASB}}$; $\text{SD} = \frac{\text{SB}}{c, \text{BSD}}$; $\text{AB} = \text{SB}\, t, \text{ASB}$; and $t, \text{CSD} = \frac{\text{CD}}{\text{SD}} = \frac{\text{AB}}{\text{SD}}$ (from the nature of the figure) $= \frac{\text{AB}}{\text{SB}}\, c, \text{BSD}$; but $\frac{\text{AB}}{\text{SB}} = t, \text{ASB}$; consequently, $t, \text{CSD} = t, \text{ASB}\, c, \text{BSD}$, or in an analogy,

$t, \text{ASB} : t, \text{CSD} :: \text{rad.} : c, \text{BSD}$; which gives the relation sought.

Cor. $\text{AC} = \text{AS}\, t, \text{ASC} = \text{SB}\, f, \text{ASB}\, t, \text{ASC} = \text{BD} = \text{SB}\, t, \text{BSD}$;

$$\text{whence } f, \text{ASB}\, t, \text{ASC} = t, \text{BSD}, \text{ or}$$
$$t, \text{ASC} = t, \text{BSD}\, c, \text{ASB}.$$

[To be continued.]

IV. *On*

IV. *On the Formation of the Bark of Trees. In a Letter from* T. A. Knight, *Esq. F. R. S. to the Right Honourable Sir* Joseph Banks, *K. B. P. R. S. &c.**

MY DEAR SIR,

An extraordinary diversity of opinion appears to have prevailed among naturalists, respecting the production and subsequent state of the bark of trees.

According to the theory of Malpighi, the cortical substance, which is annually generated, derives its origin from the older bark; and the interior part of this new substance is annually transmuted into alburnum, or sap wood; whilst the exterior part, becoming dry and lifeless, forms the exterior covering, or cortex.

The opinions of Grew do not appear to differ much from those of Malpighi; but he conceives the interior bark to consist of two distinct substances, one of which becomes alburnum, whilst the other remains in the state of bark: he, however, supposes the insertments in the wood, the "utriculi" of Malpighi, and the "tissu cellulaire" of Du Hamel, to have originally existed in the bark.

Hales on the contrary contends, that the bark derives its existence from the alburnum, and that it does not undergo any subsequent transformation.

The discoveries of Du Hamel have thrown much light on the subject; but his experiments do not afford any conclusive result, and some of them may be adduced in support of either of the preceding hypotheses: and a modern writer (Mirbel†) has endeavoured to combine and reconcile, in some degree, the apparently discordant theories of Malpighi and Hales. He contends with Hales, that the alburnum gives existence to the new layer of bark; but that this bark subsequently changes into alburnum; though not precisely in the manner described by Malpighi.

So much difference of opinion, amongst men so capable of observing, sufficiently evinces the difficulty of the subject

* From *Transactions of the Royal Society of London*, part i. for 1807.

† *Traité d'Anatomie et de Physiologie végétales.*

 they

they endeavoured to investigate: and in the course of experiments which has occupied more than twenty years, I have scarcely felt myself prepared till the present time, even to give an opinion respecting the manner in which the cortical substance is generated in the ordinary course of its growth; or reproduced, when that, which previously existed, has been taken off.

Du Hamel has shown, that the bark of some species of trees is readily reproduced, when the decorticated surface of the alburnum is secluded from the air; and I have repeated similar experiments on the apple, the sycamore, and other trees, with the same result; I have also often observed a similar reproduction of bark on the surface of the alburnum of the Wych elm (*Ulmus montana*) *in shady situations*, when no covering whatever was applied. A glareous fluid, as Du Hamel has stated, exudes from the surface of the alburnum: this fluid appears to change into a pulpous unorganised mass, which subsequently becomes organised and cellular; and the matter, which enters into the composition of this cellular substance, is evidently derived from the alburnum.

These facts are therefore extremely favourable to the theory of Hales; but other facts may be adduced which are scarcely consistent with that theory.

The internal surface of pieces of bark, when detached from contact with the alburnum, provided they remain united to the tree at their upper ends, much more readily generate a new bark, than the alburnum does under similar circumstances: a similar fluid exudes from the surfaces of both, and the same phænomena are observable in both cases. The cellular substance, however, which is thus generated, though it presents every external appearance of a perfect bark, is internally very imperfectly organised; and the vessels which contain the true sap in the bark, are still wanting; and I have found, that these may be made, by appropriate management, to traverse the new cellular substance in almost any direction. When I cut off all communication above, and on one side, between the old bark and that substance, I observed, that the vessels proceeded across it, from the old bark on the other

other side, taking always in a greater or less degree an inclination downwards; and when the cellular substance remained united to the bark at its upper end only, the vessels descended nearly perpendicularly down it; but they did not readily ascend into it, when it was connected with the bark at its lower extremity only: the result of similar experiments, when made on different species of trees, was, however, subject to some variations.

Pieces of bark of the walnut-tree, which were two inches broad, and four long, having been detached from contact with the alburnum, except at their upper ends, and covered with a plaister composed of bees-wax and turpentine, in some instances, and with clay only in others, readily generated the cellular substance of a new bark; and between that and the old detached bark very nearly as much alburnum was deposited as in other parts of the tree, where the bark retained its natural position; which, I think, affords very decisive evidence of the descent of the sap through the bark. Similar pieces of bark, under the same mode of treatment, but united to the tree at their lower ends only, did not long remain alive, except at their lower extremities; and there a very little alburnum only was generated. Other pieces of bark of the same dimensions, which were laterally united to the tree, continued alive almost to their extremities; and a considerable portion of alburnum was generated, particularly near their lower edges; the sap appearing in its passage across the bark to have been given a considerable inclination downwards: probably owing to an arrangement in the organisation of the bark, that I have noticed in a former memoir*, which renders it better calculated to transmit the sap towards the roots than in any other direction.

I have in very few instances been able to make the walnut-tree reproduce its bark from the alburnum, though under the same management I rarely failed to succeed with the sycamore and apple-tree. Pieces of the bark of the apple-tree will also live, and generate a small portion of alburnum, though only attached to the tree at their lower extremities; probably owing to a small part of the true sap being carried

* Philosophical Transactions of 1804.

upwards by capillary attraction, when the proper action of the cortical vessels is necessarily suspended.

The preceding experiments, and the authority of Du Hamel, having perfectly satisfied me, that both the alburnum and bark of trees are capable of generating a new bark, or at least of transmitting a fluid capable of generating a cellular substance, to which the bark in its more perfectly organised state owes its existence, my attention was directed to discover the sources from which this fluid is derived. Both the bark and the alburnum of trees are composed principally of two substances; one of which consists of long tubes, and the other is cellular; and the cellular substance of the bark is in contact with the similar substance in the alburnum, and through these I have long suspected the true sap to pass from the vessels of the bark to those of the alburnum*. The intricate mixture of the cellular and vascular substances long baffled my endeavours to discover from which of them, in the preceding cases, the sap, and consequently the new bark, proceeded; but I was ultimately successful.

The cellular substance, both in the alburnum and bark of old pollard oaks, often exists in masses of near a line in width, and this organisation was peculiarly favourable to my purpose. I therefore repeated on the trunks of trees of this kind, experiments similar to those above mentioned which were made on the walnut-tree.

Apparently owing to the small quantity of sap, which the old pollard trees contained, their bark was very imperfectly reproduced; but I observed a fluid to ooze from the cellular substance, both of the bark and alburnum; and on the surface of these substances alone, in many instances, the new bark was reproduced in small detached pieces.

I have endeavoured to prove in former communications†, that the true sap of trees acquires those properties which distinguish it from the fluid recently absorbed, by circulating through the leaf; and that it descends down the bark, where part of it is employed in generating the new substances

* Phil. Trans. 1805, page 14.

† Id. for 1801, 1805, and 1806.

annually

annually added to the tree; and that the remainder, not thus expended, passes into the alburnum, and there joins the ascending current of sap. The cellular substance, both of the bark and alburnum, has been proved, in the preceding experiments, to be capable of affording the sap a passage through it; and therefore it appears not very improbable, that it executes an office similar to that of the anastomosing vessels of the animal economy, when the cellular surfaces of the bark and alburnum are in contact with each other; and, when detached, it may be inferred, that the passing fluid will exude from both surfaces: because almost all the vessels of trees appear to be capable of an inverted action in giving motion to the fluids which they carry.

As the power of generating a new bark appeared in the preceding cases to exist alike in the sap of the bark and of the alburnum, I was anxious to discover how far the fluid, which ascends through the central vessels of the succulent annual shoot, is endued with similar powers. Having therefore made two circular incisions through the bark, round the stems of several annual shoots of the vine, as early in the summer as the alburnum within them had acquired sufficient maturity to perform its office of carrying up the sap, I took off the bark between these incisions; and I abraded the surface of the alburnum to prevent a reproduction of it. The alburnum in the decorticated spaces soon became externally dry and lifeless; and several incisions were then made longitudinally through it. The incisions commenced a little above, and extended below the decorticated spaces, so that, if the sap of the central vessels generated a cellular substance (as I concluded it would), that substance might come into contact and form a union with the substance of the same kind emitted by the bark above and below.

The experiment succeeded perfectly, and the cellular substances generated by the central vessels, and the bark, soon united, and a perfect vascular bark was subsequently formed beneath the alburnum, and appeared perfectly to execute the office of that which had been taken off; the medulla appeared to be wholly inactive.

I have already observed, that the vessels, which were ge-

nerated in the cellular substance on the surface of the alburnum of the sycamore and the apple-tree, traversed that substance in almost every direction; and the same thing appears to occur beneath the old bark, when united to the alburnum. For, having attentively examined, through every part of the spring and summer, the formation of the internal bark, and alburnous layer beneath it, round the bases of regenerated buds, which I had made to spring from smooth spaces on the roots and stems of trees, I found every appearance perfectly consistent with the preceding observations. A single shoot only was suffered to spring from each root and stem, and from the base of this in every instance the cortical vessels dispersed themselves in different directions. Some descended perpendicularly downwards, whilst others diverged on each side, round the alburnum, with more or less inclination downwards, and met on the opposite side of it. The same pulpous and cellular substance appeared to cover the surfaces of the bark and alburnum, when in contact with each other, as when detached; and through this substance the ramifications of the vessels of the new bark extended themselves, appearing to receive their direction from the fluid sap which descended from the bark of the young shoots, and not to be, in any degree, influenced in their course by the direction taken by the cortical and alburnous vessels of the preceding year.

Whenever the vessels of the bark, which proceeded from different points, met each other, an interwoven texture was produced, and the alburnum beneath acquired a similar organisation: and the same thing occurs, and is productive of very important effects, in the ordinary course of the growth of trees. The bark of the principal stem, and of every lateral branch, contains very numerous vessels, which are charged with the descending true sap; and at the juncture of the lateral branch with the stem, these vessels meet each other. A kind of pedestal of alburnum, the texture of which is much interwoven, is in consequence formed round the base of the lateral branch; which thus becomes firmly united to the tree. This pedestal, though apparently a part of the branch, derives a large portion of the matter, annually added

to

to it, from the cortical vessels of the principal stem; and thence, in the event of the death of the lateral branch, it always continues to live. But it not unfrequently happens, that a lateral branch forms a very acute angle with the principal stem, and, in this case, the bark between them becomes compressed and inactive; no pedestal is in consequence formed, and the attachment of such a branch to the stem becomes extremely feeble and insecure*. Instead of the reproduced buds of the preceding experiment, buds were inserted in the foregoing summer, or attached by grafting in the spring; and, when these succeeded, though they were in many instances taken from trees of different species, and even of different genera, no sensible difference existed in the vessels, which appeared to diverge into the bark of the stock, from these buds and from those reproduced in the preceding experiments.

It appears, therefore, probable, that a pulpous organisable mass first derives its matter either from the bark, or the alburnum: and that this matter subsequently forms the new layer of bark; for, if the vessels had proceeded, as radicles†, from the inserted buds, or grafts, such vessels would have been, in some degree, different from the natural vessels of the bark of the stocks; and it does not appear probable, even without referring to the preceding facts, that vessels should be extended, in a few days, by parts successively added to their extremities, from the leaves to the extremities of the roots; which are, in many instances, more than two hundred feet distant from each other. I am, therefore, inclined to believe, that, as the preceding facts seem to indicate, the

* The advantages, which may be obtained by pruning timber trees judiciously, appear to be very little known. I have endeavoured to ascertain the practicability of giving to trees such forms as will render their timber more advantageously convertible to naval or other purposes. The success of the experiments on small trees has been complete, and the results perfectly consistent, in every case, with the theory I have endeavoured to support in former memoirs; and I am confident that, by appropriate management, the trunks and branches of growing trees may be moulded into the various forms best adapted to the use of the ship-builder; and that the growth of the trees may at the same time be rendered considerably more rapid, without any expense or temporary loss to the proprietor.

† Darwin's Phytologia.

matter,

matter, which composes the new bark, acquires an organisation calculated to transmit the true sap towards the roots, as that fluid progressively descends from the leaves in the spring: but whether the matter, which enters into the composition of the new bark, be derived from the bark or the alburnum, in the ordinary course of the growth of the tree, it will be extremely difficult to ascertain.

It is, however, no difficult task to prove, that the bark does not, in all cases, spring from the alburnum; for many cases may be adduced in which it is always generated previously to the existence of the alburnum beneath it: but none, I believe, in which the external surface of the alburnum exists previously to the bark in contact with it, except when the cortical substance has been taken off, as in the preceding experiments. In the radicle of germinating seeds, the cortical vessels elongate, and new portions of bark are successively added to their points, many days before any alburnous substance is generated in them; and in the succulent annual shoot the formation of the bark long precedes that of the alburnum. In the radicle the sap appears also evidently to descend* through the cortical vessels†, and in the succulent annual shoot it as evidently passes up through the central vessels‡, which surround the medulla. In both cases a cellular substance, similar to that which was generated in the preceding experiments, is first formed, and this cellular substance in the same manner subsequently becomes vascular; whence it appears, that the true sap, or blood of the plant, produces similar effects, and passes through similar stages of organisation, when it flows from different sources, and that the power of generating a new bark, properly speaking, belongs neither to the bark nor alburnum, but to a fluid, which pervades alike the vessels of both.

I shall, therefore, not attempt to decide on the merits of the theory of Malpighi, or of Hales, respecting the repro-

* Phil. Trans. 1805 and 1806.

† I wish it to be understood, that I exclude in these remarks, and in those contained in my former memoirs, all trees of the palm kind, with the organisation of which I am almost wholly unacquainted.

‡ Phil. Trans. 1805. Mirbel has called the tubes, which I call the central vessels, the "tissu tubulaire" of the medulla.

duction

duction of the interior bark; but I cannot by any means admit the hypothesis of Malpighi and other naturalists, relative to the transmutation of bark into alburnum; and I propose to state my reasons for rejecting that hypothesis, in the next communication I have the honour to address to you. I am, my dear sir,

Your much obliged obedient servant,

T. A. Knight.

Elton,
Dec. 18, 1806.

V. *Observations upon the crystallized Bodies contained in Lava. By M.* G. A. De Luc.

[Concluded from vol. xxvii. p. 312.]

Judging from volcanic phænomena, what depth may we presume the fires to be, compared with the diameter and the mass of the globe? This depth is more or less considerable, without doubt, according to the mass of the volcano that is elevated: thus it is very probable that the fires of Etna, of the peak of Teneriffe, and of the volcanoes of Peru, are deeper than those of Vesuvius, Vulcano, and Stromboli. This is all we can venture to say, but we cannot hazard any thing upon the causes or events which have conduced to the formation of our globe.

"Man separates, dissolves, concentrates and combines the minerals, producing changes in their forms:" all this is true; he does so by solvents, and by the fire of his furnaces. But it is not added that he cannot again by any means recall them to their state of mineralisation, by any fire whatever. Neither can we regenerate the vegetables which we burn and reduce to ashes; nor can the chemist reproduce the substances he decomposes: we are therefore very far from being able to produce any thing similar to the rocks, the crystals, and the minerals of our mountains. This reflection alone overturns every system which maintains that these substances are formed by fire, since all the operations both of natural and artificial fire, with which we are acquainted, can produce nothing like them.

These

These limits, which the resources of man cannot pass, should render us very circumspect in the results we attribute to them, since none of the substances in nature, which man destroys or alters, can again appear, except by pursuing the laws and the course established by the Creator from the origin of things.

M. Fleuriau de Bellevue mentions a singular production of a lime furnace, which he quotes as an example in favour of his system: "This production," he says, "resembles in its interior certain horny rocks of the Alps, and some compact and homogeneous lava. Its exterior is full of bubbles like those of lava, its surface is covered with a yellow enamel, and its cavities are overspread with small crystals:" and he adds in a note, "Let it not be supposed that these kinds of stones have fallen accidentally into these furnaces; that is impossible."

I shall not make any direct objection on this head; I must be first acquainted with the production alluded to, and particularly with the neighbourhood of the lime kiln in question. But I shall offer one general observation, which may throw some light upon its origin. Examples are frequent of there being introduced among the broken calcareous stones, fragments of other stones, to which the workmen pay no attention, and which are only perceived when the lime is extracted: there is then found, instead of a piece of lime, a stone covered on its surface with a vitreous varnish, which on being broken shows granite, serpentine, or any other vitrescible rock: these examples are frequent in the lime kilns in our neighbourhood. In order to be convinced that no such fragments can be introduced, it must first be proved that there are no calcareous rocks in the country exempt from quartzose or siliceous nuclei, and that there are no other kinds of rocks, belonging to the soil, or adventitious. It is also very probable, and I am persuaded of it from a great number of examples, that the production of the lime kiln mentioned is owing to some other kind of stone than the species intended to be calcined in it.

"The naturalists," M. Fleuriau de Bellevue proceeds, "who still think that the rocks upon which the volcanic fires have

have acted have undergone an incomplete fusion only, and that their crystals remained untouched in the middle of their fluid paste, are obliged to have recourse to a multitude of suppositions, in order to explain the state in which lavas are when cooled."

These naturalists have recourse to no such supposition. Nothing changes the form or the nature of lava after it has cooled. The foreign substances which it contains in its burning paste, remain in the same form; no changes are produced, the fire of the volcanoes not having had power sufficient to fuse or alter them. I have given a great number of examples of this.

I shall call on this occasion to the recollection of my readers, the idea I presented in my former memoir, upon the probable state in which the subterranean volcanoes were from whence the lava issued. We see that, in order to reduce the rocks and the minerals to fusion, they must be broken into pieces: nevertheless there are no stampers or grinding machines in the strata where the lavas take their origin, and the volcanic fires, any more than those of our furnaces, cannot melt rocks in large masses. One would think, therefore, that in such beds the chemical affinities may be exercised, and form isolated and grouped crystals which remain enveloped in the matter in fusion. How does this fusion take place, and whence proceed the fires that produce it? We see from the various emanations that sulphur is the chief ingredient, and that iron enters into the mixture; that the marine acid and sal-ammoniac also form parts of the mixture: but what circumstance, what combination is requisite to excite the fermentation which produces the fires, the fusions, and the other volcanic phænomena? Upon this we can only form conjectures, some of which may approach nearer to the truth than others. But as all the means in our power can neither hinder nor produce any thing, it is of little importance to know upon what our conjectures are founded as to the origin of these fires, and the manner in which they act. What is essentially requisite, is, to take care not to give them a wider influence, nor to attribute more action to them, than they really

really have, that we may not found systems upon mistakes or exaggerations.

M. Fleuriau de Bellevue does not admit that sea water is absolutely necessary for producing volcanoes; and he quotes against this opinion, which seemed at first very alluring, a volcanic eruption related by Messrs. Humboldt and Bonpland, which took place, in 1759, " in a plain of Mexico, *forty leagues from the sea in a straight line*; an eruption which elevated in one night a volcano 1494 feet high, surrounded by more than two thousand mouths, which still smoke."

If burning volcanoes could be manifested any where otherwise than under the influence of the waters of the ocean, we should not need to confine ourselves to a single example; a great number in that case would certainly be produced; and if this were the case, the opinion I have hazarded would never have come into my mind. But after having directed my attention to the great fact, that there is no burning volcano whatever in inland districts; that no expanse can be produced of fresh water, however vast it may be, having ever produced any volcano; that all of them are near the sea, or encompassed by it; and having observed that the smoke of volcanoes deposits marine acid in abundance; I drew this unavoidable conclusion from all these circumstances, that sea water is absolutely necessary, from the salts it holds in solution, for producing the fermentations which raise and keep up volcanoes.

This conclusion has been since confirmed by the eruptions of water from the volcanoes of Iceland, which deposit marine salt in great abundance; and lately also by an observation of Messrs. Humboldt and De Buch, who were witnesses of the eruption of Vesuvius in the month of August 1805, and saw the sides of a crevice of its crater covered with a crust of muriate of soda two or three inches in thickness.

From which it results that the fact quoted by M. Fleuriau de Bellevue proves nothing, if not that there are subterranean galleries which extend 40 leagues from the sea, and that in this instance its waters have penetrated that length. If all the circumstances accompanying this fact were well known, it

it is very probable that an explanation would be derived from them more explicit still. In 1538 an eruption equally sudden raised the *Monte Nuovo* near Naples.

"All those who have seen volcanoes in activity," says M. Fleuriau de Bellevue, "relate that nothing can equal the violence and immensity of their fires, and nevertheless some people reduce their powers to those of our common furnaces."

A volcano in a state of eruption presents a spectacle so grand and imposing, that it seizes upon the imagination of the spectator, and throws him into astonishment. This is an effect of the extent of its fires, of the noise that accompanies them, and of the spectacle of the currents of burning lavas. But when we look with the eye of a philosopher, we soon judge, from its effects, that this grand furnace has not in each of its points an intensity of heat so great as that produced in our melting furnaces. It may be easily conceived why our furnaces have this greater intensity of heat. It is produced by the continual currents of air introduced into them, which by their extreme rapidity incessantly bring with them a new blast, the presence of which gives a greater activity to the fire. This does not happen to the furnaces of volcanoes which cannot have a communication, necessary for this great activity, with the atmospheric air. This is the reason why we see reduced to the state of glass, in a crucible in our melting furnaces, the pyroxene schorl, which is unalterable in the fires of volcanoes; and we also obtain a complete vitrification of the fragments of lava which we subject to the same experiments.

Obsidian, or the compact glass of volcanoes, is that part of their materials which has undergone the greatest degree of heat. The vitrification of obsidian is complete; none of the pieces I ever saw exhibit any thing except glass, all the substances composing it having been reduced to a perfect fusion. These vitreous pieces proceed, therefore, from a very strong heat produced in the volcano by some particular circumstance.

Why do not these melted obsidians, which must cool as slowly as the other lavas, present in their interior any crystal-

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lized form, if it is not that, because, all the substances having been melted, there can be nothing except glass in their whole mass?

I may even add, that it is much more extraordinary that we should, from the operations of our trifling workshops, infer the degree of power of the fires of volcanoes, and thus ascribe to them an unlimited extent; and more extraordinary still, hence to conclude the origin and formation of the primordial rocks and mountains. Let us restrict ourselves to the effects produced by our own resources; and not throw ourselves into a labyrinth of illusions, by concluding from small effects to great ones; for, our means being only artificial, they are not those which nature employs.

"The naturalists," M. Fleuriau de Bellevue proceeds, "who think that the crystals contained in lava remained untouched in the midst of their fluid paste, pass over in silence the observation of those who, as Spallanzani and Hubert relate, have seen the *lava gush out at different times as water gushes out of a fountain, and form a multitude of very brisk rivulets*; and lastly possess a degree of fluidity sufficient for introducing itself into the smallest interstices of the bodies it penetrates:" and he adds in a note, "M. Faujas has in his collection a piece of a palm tree from the island of Bourbon, which proves that the fluidity of the lava has been very great, since it was introduced even into the fibres of wood."

An impossibility would result from this fact, if it were true, i. e. that there might be lavas in fusion without incandescence; for a body which is so combustible as a piece of palm tree, or any other vegetable, would have been burnt and consumed, or reduced into charcoal, on the first contact of the lava. Here there is a mistake, therefore. Either the matter which surrounds the piece of palm tree is not lava, or the matter surrounded is not a vegetable. I have read in the account of a voyage to Iceland, translated and published at Paris in 1802, that the Danish travellers thought they saw wood in a piece of the lava of Hecla. Count de Borch has

has made the same mistake and a much greater, when he says, he saw *pieces of wood slightly scorched* in entire rocks of the lava of Etna.

I am in possession of a large piece of vitreous lava which I brought from the island of Vulcano, which may explain this illusion. It has great bubbles, which are much elongated by the flowing of the lava, and their surface is traced with thready fissures, having the appearance of ligneous fibres, increased by the hue which the fumes have given them that are exhaled constantly from the substance in fusion. Several people who have seen this lava took it at first for wood. I have another vitreous piece of lava from Lipari, the matter of which is drawn out into threads so minute and so close that no agatized fossil wood, the fibres of which are very distinct, has more the appearance of wood than this piece has, nor has it a vitreous lustre. I have another specimen, vitreous also, one of the surfaces of which, having been the outer one, is traced with a multitude of very small threads, arranged in some places in undulations, like the ligneous fibres round a knotty piece of wood.

From these examples I was led to believe that the specimen from the Island of Bourbon is entirely lava, with a ligneous appearance upon one of its faces: because in every such case, a vegetable, even in the state of wood, after its combustion, which is inevitable, can only leave a void space in the lava and traces of charcoal, but never the impression of fibres, and far less the fibres themselves.

In order to lay a foundation for his favourite opinion, M. Fleuriau de Bellevue hazards several reasonings, which I shall not follow, because facts and not conjectures are wanting. One of his arguments is the following: "The large pieces of lava, which perform the first part in the eruptions of volcanoes, dart out of the crater itself, as well as from the sides of the mountain or its base. They come rapidly out of the fires of the volcano also, being incomparably hotter than the substances which remain at rest in the crater. This immense heat and this rapidity *cause them to be squirted out and to flow down like water*, and cannot admit

of crystals being formed. All the crystals found among them are produced during the cooling of the lava."

I must remark in the first place, that the expressions so often repeated, of *the lava gushing and flowing out like water*, are merely metaphorical; for, so far from lava in any case flowing like water, it successively leaves all its matter fixed upon the ground over which it passes.

M. Fleuriau de Bellevue surely does not recollect the lava of Etna of 1669, which I have already quoted.

This lava, which came out of the base of this great volcano, extended over some leagues in length, advanced into the sea, where it accumulated into prodigious heaps, after having covered its route with an enormous extent of its matter both in breadth and thickness. This is certainly one of those lavas which hold the first rank among volcanic eruptions. But I repeat that this lava, the fragments of which I have before me, is filled throughout its whole course, from its leaving the crater to the extremity of its destructive track*, with a multitude of pyroxene schorls of these whitish crystalline laminæ I have described, and with a great number of small chrysolites, and the crater whence they came has vomited out myriads of these same substances. Do we see then any formations effected at the time of the cooling of the lava, when all its crystals existed in it at the time of its greatest fusion and incandescence, the very fire of the erup-

* There is an account published of this dreadful eruption, made at the time by the earl of Winchelsea, who on his return by sea from his embassy to Constantinople, halted 24 hours at Catana and was an eye-witness of it. The lava which was then called the *torrent of fire* had reached the sea. "*By its horrid devastation and rapid progress, this torrent was called,*" says the ambassador, "*an inundation, a deluge of fire.*" It was precipitated into the sea, with a horrible noise; the boiling of the water, the thick vapours and dreadful hissings, and the burning substance swhich continually flowed down, all contributed to increase the horror of the scene. No human means being able to arrest the flames which this devastating torrent produced in its progress, it destroyed the habitations of 27,000 persons. It flowed along the walls of Catana; the inhabitants saved themselves with their most valuable effects. The city, however, was preserved; but its walls and bastions on the side on which the lava flowed, which was the south, were buried under it. Parts of them are still seen through some large fissures. The appearance of these enormous piles of black rocks creates the idea of chaos and desolation.

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tion having vomited out by its crater an innumerable multitude of isolated crystals?

These naturalists were perfectly right, who remarked that the leucites and pyroxene-schorls are crystals which do not exist in the beds we are able to see; and they are right also in drawing the conclusion from it, that these substances would have been for ever unknown to us, if the volcanic eruptions, in bringing them to light, had not made us acquainted with them. M. Fleuriau de Bellevue thinks that this is a *supposition*. Nothing, however, can be more true than the observation, and nothing is more natural than the inference drawn from it.

"We have seen," he continues, "that no example whatever proves that the aqueous solutions ever formed, or could form, rocks similar to primitive rocks; and that fire, on the contrary, presents us with products every day, which are not only analogous to them, but even identical."

We see, on the contrary, that the productions of fire have only an apparent, but no real resemblance to the primitive, or, to speak more precisely, to the *primordial* rocks. The volcanic fires, any more than those of our furnaces, have never produced, and never will produce, any thing similar, because the primordial beds do not owe their origin to fire.

Neither can aqueous solutions form any such strata; they were formed by precipitations in the primordial fluid at epochs near to the time of the origin of the globe, and every thing shows that they are not formed at present; the water of the modern ocean does not now contain the elements necessary for this purpose, it is now entirely devoid of them. The slime of rivers, which it has been thought would form them, does not reach the bottom of the sea; the tides drive it back and retain it at the mouths of the rivers, where there are daily fresh additions made to the first shores of continents.

I shall here repeat a remark I have frequently made. These additions are so little to be compared with the extent of the seas, that they cannot produce any sensible change in their level. These alluvia of rivers have been often quoted and mistaken for monuments of the retreat of the sea.

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By what mark can we distinguish the ancient volcanoes throughout the world, except by their form, and the nature of the substances that characterize them? In truth, they are completely distinct from all other mountains. The Alps, Appenines, Jura, and the Pyrenees have no resemblance at all to volcanic mountains; the strata and substances of the former having been formed in the waters, and fire having had nothing to do in their formation.

It was these distinctive and invariable characters of volcanoes, and of the soil which surrounds them, that suggested the following expressions in my former observations. "When the valley of Quito and the mountains skirting it are viewed by naturalists skilled in the knowledge of volcanoes and volcanic substances, they will acknowledge that the state of things is what I have mentioned!" I should have been far from expressing myself in this manner, if other soils and other mountains were in question. But the great mountains which skirt both sides of this celebrated valley being certainly volcanoes, three of which are still burning, and it's soil being composed of their enormous dejections, I could propose this opinion without going too far.

The antient volcanoes, which are observed at the surface of some continents, are not so numerous as M. Fleuriau de Bellevue thinks, when he says that volcanoes, whether burning or extinguished, are to be found every where throughout the surface of the globe. This is a great exaggeration: a great number of them are to be seen in various places, without doubt, but the space they occupy is not to be compared with that where there are none. The same remark applies to old and extinguished volcanoes; for the burning volcanoes are in very small number. There are only four in Europe: that of Iceland is in a very remote latitude.

This notion brings to my recollection a similar one of M. Patrin, which he applies to Italy. It is to be found in his Inquiries concerning Volcanoes upon the Principles of pneumatic Chemistry. "Italy," says M. Patrin, "is covered with volcanoes, lava and tufo of an enormous thickness." Nevertheless, it results from the real state of Italy, that the Appenines

Appenines which traverse it from one end to the other, all the ramifications of this chain, and all the eastern shores of this peninsula, have nothing volcanic; that the soils of this kind are exclusively upon the western coast, separated frequently by aquiformed strata.

When the explanations upon the method in which a physical terrestrial fact, surrounded by any veil, can happen, are removed from the sense which is the most natural and the most conformable to that which is dictated by a similar phænomenon, they may be very different from each other, and even completely contradictory. Thus it may have happened that M. Patrin, being equally of opinion that the pyroxene schorls were not pre-existent to the lava, separates them from the matter of lava, and makes them spring from an aëriform fluid *which has passed to a solid consistence by the effect of attraction.*

From all the facts I have established, it may be received as a final conclusion* :—

That every burning volcano, antient or modern, whatever be its height and extent, and wherever it is situated, is a mountain belonging to a class distinct from all others: that it is not formed by any marine stratum; that all the mass of substances composing it are productions of fire; that it has been raised from its base to its summit by the accumulation of substances successively vomited out by its eruptions, the focus of which is below all the strata we are acquainted with.

That the crystallized bodies contained in lava are foreign to them; that they have been formed previously by the humid way, in strata which the volcanic fires have brought into fusion, leaving untouched those crystals, because the fires were not intense enough to melt them.

That we should cease to assert that volcanoes display themselves at the summits of mountains, because the whole body of the volcanic mountains constitutes the volcano.

* M. de Luc exhibited to the Natural History Society of Geneva the volcanic productions, and those of the mineral strata, from which he drew his observations.

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This is shown by the new mouths which are so frequently opened in their sides and at their bases.

That the water of the sea is absolutely necessary, from the salts it holds in solution, for exciting those fermentations which produce volcanoes.

That all the substances and the strata which compose the calcareous, schistous and granitic mountains, and all their varieties; the sandy gypsose and argillaceous mounts are the work of water, and fire has never approached them.

That all the old volcanoes, which are in the middle of the earth, have burned under the water of the sea. The schists and granites, which are seen around some of them, are foreign to them; they are strata through which their eruptions have forced a passage, and which remain exposed to view. They would have been buried under volcanic matters never to appear again, if these volcanoes had been longer in activity. Those which were burning at the moment when the sea retired from above our continent, ceased to burn at that period;—an epoch now beyond the memory of the inhabitants of any country, because none existed upon the soil surrounding these volcaeocs, which was then below the sea.

Among the numerous facts which prove this truth, count Marzari of Vicenza furnished me with a very remarkable one upon his return from a journey he made in Auvergne. At Santourgue there was found a stratification of volcanic sand of about six inches in thickness, between two calcareous strata. It has therefore happened that, after a first calcareous deposit upon the flanks or the base of the volcano, an eruption has broken out and extended this sand, upon which a new calcareous deposit has been formed,—operations which could take place in the sea only. Count Marzari had the goodness to give me a specimen of this sand, which is similar to that which was thrown out from the upper mouth of Etna in the eruption of 1763, which I have before mentioned.

I shall here repeat what I have often said: That it is necessary, in order to distinguish the different epochs at which volcanoes have burned, and in order not to confound them,

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to call *antient,* such volcanoes as have burned in the sea before our continents were laid bare; and to call *extinguished*, those only which from their situation might be burning still, if in the inflammable matters which fed them were not burnt out.

I have now concluded my observations. I have had no other object in making these observations, than to explain in a clearer manner the great phænomena of volcanoes, that we may not attribute effects to them in which they have no share, nor refuse to allow them such as they really produce. These limits, founded upon well ascertained facts, are able of themselves to banish the systems founded upon contrary notions, and to give more certain bases to geology, that branch of knowledge so important in terrestrial physics.

VI. *Account of some additional Experiments made by the Galvanic Society of Paris. Communicated by M.* RIFFAULT *.

THE Galvanic Society had announced, that although they were anxious to verify the fact advanced by Pacchiani, of the formation of the muriatic acid in distilled water when submitted to the galvanic action, yet that no satisfactory result had been hitherto obtained. The Society, in publishing the detail of their experiments, in the pursuit of which they rigorously conformed themselves to the course pointed out by M. Pacchiani himself, manifested an intention of repeating and varying these experiments in quite a different manner, before drawing any conclusion upon so important a discovery. It was in these circumstances that M. Brugnatelli, one of the corresponding members of the Society, transmitted from Pavia some observations which he had communicated to the National Institute of Italy†, containing an account of his experiments upon the same subject, which had completely succeeded, but under certain modifications in the manner of proceeding. The Society, after having been made acquainted with this memoir, ascertained that in the appara-

* From *Annales de Chimie,* tom. lx. p. 113.

† See Phil. Mag. vol. xxv. p. 57 and 133.

tus he adopted, M. Brugnatelli having avoided the employment of every substance foreign to the water used in the experiment, and to the metallic conductors, the results which might be expected by his process could not any longer present any cause for uncertainty. The Society consequently decided upon following M. Brugnatelli's process, in the experiments they were about to make. Accordingly, on the 8th of May last, the class for physical inquiries of the Society having assembled, they arranged their apparatus in the following manner.

They filled with distilled water, proved by a solution of nitrate of silver, a tube of glass 60 millimetres in length* and about 7 millimetres of interior diameter. A glass syphon of about 5 millimetres of interior diameter, each of the branches of which was 40 millimetres long, with a space of 15 millimetres between them, was also filled with the same distilled water, and rested in such a manner that one of its branches entered into the tube and the other into a glass full of distilled water, placed near the tube at the distance determined by the branches of the syphon. They introduced into the tube a gold wire about one fourth of a millimetre, or 0·976, and into the glass a strip of very thin tin foil brought as near as possible to the branch of the syphon, which plunged into it. Every thing being thus prepared, a pile was mounted, composed of one hundred pair of square plates zinc and copper, 80 millimetres surface each (about three square inches), separated by pieces of cloth, saturated with a strong solution of muriate of ammonia. The communication was established by the gold wire corresponding with the zinc pole, and by the strip of tin foil adjoining to the copper pole. The activity of this pile was soon displayed, and was kept up about 60 hours, when it ceased to all appearance; the pile was dismounted, without changing any thing in the rest of the apparatus, and another was substituted for it, composed in the same manner, but with 60 pair of plates only. The action of this second pile was not entirely extinguished, when we thought it right to stop the experiment, in order to examine the results. On

* The French millimetre is = ·039·37 inch English.

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emptying the water from the tube into a glass previously well washed with distilled water, we were instantly struck with a very decided smell of oxymuriatic acid. The tube when emptied had the same smell, and water when poured into the glass acquired it also in a very marked manner. This water had no bad taste, but it reddened turnsole tincture very sensibly. Some drops of a solution of nitrate of silver immediately produced a very perceptible cloud, followed by a precipitate very distinctly formed of muriate of silver, in the water of the syphon and of the glass; it did not act upon turnsole paper, and produced nothing with the solution of nitrate of silver, except a cloud scarcely perceptible, but without forming any precipitate. The water of the glass assumed alone, after resting some hours, a slight rose-coloured hue.

The Society thought themselves bound to abstain from drawing any conclusion from the result of this first experiment, but they felt how important it was to confirm them by new experiments. A great number have been consequently since made in the same manner, but with piles composed of a more or less considerable number of pairs of disks, of a more or less energetic action, and continued for a longer and shorter period. In all these experiments the water of the tube always manifested the same clouds, and the same distinct precipitate of muriate of silver, with the solution of nitrate of silver. In some of the experiments, the tincture of turnsole was not changed into red; and in one only, where the activity of the pile had been kept up about 100 hours, the smell of oxymuriatic acid was much more remarkable, also much more pungent than in the first experiments. In one of these experiments, the water of the glass into which the strip of tin foil was inserted, presented something like an alkaline trace. And on pouring a portion into a tincture of turnsole, which had been reddened by the water of the tube, it restored the colour of it.

Although the Society had constantly obtained many similar and decisive results, yet they did not think right to discontinue their labours. They proceeded with their experiments, even adding new precautions, in order to remove the water

water submitted to the galvanic action from the contact of the hands, and from all influence of the surrounding atmosphere. They placed under a bell glass, the tube, the syphon and the glass containing it, as well as the conducting wires of gold and the tin foil, in such a manner that they were prolonged at the exterior by passing under the bell glass. They arranged without touching any thing, and without the aid of any foreign body, except glass tubes washed in distilled water, the syphon communicating with the glass tube; in short, an experiment was never commenced until the apparatus had been disposed with the greatest possible precaution. They left the apparatus, well covered with its bell glass, upon a table in a room far removed from that in which the piles were mounted; and these last were afterwards brought ready prepared, at the moment the communication was to be established. The whole remained in this state near a window, which was generally open, in a place where no kind of operation whatever was going on, and where there was no chemical substance whatever.

The experiments made in this manner produced the same results as to the colouring the tincture of turnsole red, and as to the precipitate of muriate of silver by the solution of nitrate of silver; but the smell of oxymuriatic acid was not obtained, which had shown itself several times; and the water of the glass into which the strip of tin foil entered, gave no alkaline traces. The water of the tube alone constantly appeared to have evidently changed its state, that of the syphon and the glass remaining the same.

On uniting all these experiments, and on considering the results obtained from them, the Galvanic Society does not hesitate to conclude (always supposing that all the precautions mentioned in the above experiments have been attended to):—

1st, That distilled water submitted to the galvanic action evidently undergoes a change of state in the vessel, where the oxygen is disengaged by the conducting wire communicating with the positive pole.

2d, That water, in this new state, constantly presents the genuine characters of the muriatic acid.

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3d, That distilled water, submitted to the same galvanic action in the vessels where the hydrogen is liberated by the conducting wire, corresponding with the negative pole, is not sensibly altered.

4th, That water, in this case, gives no sign of muriatic acid.

5th, (Lastly). That distilled water submitted to the same galvanic action in the syphon, where no disengagement either of hydrogen or oxygen takes place, does not seem to have changed its state.

Although these consequences seem to the Society to be indisputable, yet they still continue to repeat their experiments upon a larger volume of water and with longer continuance, in order to satisfy the requests that have been made to them by several intelligent chemists.

VII. *Account of an Experiment made by the Galvanic Society of Paris, upon the Formation of the Oxymuriatic Acid, and the Separation of Soda from the Muriate of Soda, by means of the Pile of Volta. Communicated on the* 15*th of Dec.* 1806, *to the Galvanic Society of the French National Institute. By M.* CHOMPRE*.

In a recent number of the *Annales de Chimie**, the Galvanic Society published the experiments they made, in order to verify those of M. Brugnatelli, one of their correspondents. They concluded, 1st, That if we submit to the action of the pile of Volta, distilled water contained in two vessels communicating together by means of a syphon, the water which receives the gold wire proceeding from the positive pole, constantly presents characters which announce the presence of muriatic acid: 2d, That the water which receives the gold wire corresponding with the negative pole is not sensibly altered.

It has been observed, that when the apparatus is covered with a bell glass, to preserve it from a full communication

* See the preceding article.

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with the atmosphere and the surrounding bodies, the muriatic acid is more feebly manifested: this, perhaps, authorises the supposition that no trace of it at all would be developed if this communication were totally intercepted. The Society were anxious to obtain still greater effects. But the conductors act slowly upon pure distilled water; and in order to attain in this way results of some importance, it would be necessary to protract the experiment a very long time.

M. Pacchiani, who first announced the fact in question, made another trial with another view. In writing to M. Comparini, his countryman, and member of the Galvanic Society, he thus expresses himself:—"In the ordinary apparatus, instead of simple distilled water, I pour distilled water in which muriate of soda is dissolved. The gold wire, which enters this tube, communicates with the negative pole. Another gold wire, proceeding from the positive, touches the water of the reservoir into which the tube is inserted. An abundant disengagement of hydrogen then takes place in the tube, which communicates with the negative pole: some time afterwards the neutral solution of muriate of soda becomes an alkaline solution of soda, and no trace of muriate nor of muriatic acid is found in it."

The Galvanic Society has repeated this experiment, following the same process, and upon a small quantity of a solution of muriate of soda. The liquor of the tube communicating with the negative pole was in fact reduced to a solution of pure soda, changing the tincture of violets to green. They neglected to try the water of the vessel on the side of the positive pole.

The committee of the society were then anxious to try, in one experiment upon greater volumes, the formation of the oxymuriatic acid in pure distilled water on the side of the positive pole, and the separation of soda in a solution of muriate of soda on the side of the negative pole. The following are the details of this experiment:

In 400 grammes of distilled water, proved by the nitrate of silver, 100 grammes of muriate of soda were dissolved, carefully prepared by M. Riffault, one of the members of the committee. Fifty

Fifty grammes of this solution were poured into a glass tube closed at one end, washed in distilled water, and of 10 millimetres of interior diameter, and 144 millimetres long; in a second tube, longer by about 16 millimetres, nearly 50 grammes of pure distilled water. The first tube was placed upon a kind of chandelier of tin, in such a manner as to be upon a level with the second; and both of them were fixed with wax, the one near the other, upon a glass plate placed upon a large porcelain one. The communication was established between them by means of a syphon, the branches of which entered 27 millimetres into the tubes. In order to prevent the fingers from touching the water of the apparatus, by means of a glass rod carefully washed, they held upon one of the orifices of the syphon filled with water, a damper of paper, which they withdrew, by means of the same glass rod, at the moment when the branches of the syphon entered into the water of the tubes. Two gold wires, proved by M. Darcet, and of the diameter of about half a millimetre, had been inserted to a depth of about 65 millimetres, or thereabouts, into the two tubes, which were exactly filled. This apparatus was covered by a bell glass resting upon the large porcelain plate, and the gold wires were bent so as to come out from below the bell glass.

There were afterwards brought to the table upon which the apparatus stood, three piles, placed in communication, composed of zinc and copper, with interposed pieces of cloth saturated with a solution of muriate of ammonia. Two of these piles were formed, each of 30 pairs, 67 millimetres in diameter; the third consisted of 40 pairs of square plates, of 54 millimetres of surface. These piles had been mounted in a separate chamber, in order to prevent all influence of their atmosphere upon the liquids of the apparatus.

The gold wire on the side of the solution of the muriate of soda was placed in contact with the negative pole, and that of the pure distilled water with the positive pole. Hydrogen gas was immediately evolved in great abundance in the solution of muriate of soda. The oxygen gas was extricated less briskly in the tube containing pure water, but nevertheless with more promptitude and abundance than if

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the other tube had also contained distilled water only. This extrication of gas continued in a sensible manner for sixty hours, at which time three piles, completely similar to the first, were substituted in place of them. The energy of these new piles seemed to be extinguished in 18 hours; but they revived and were manifested for 18 hours more. The apparatus was therefore submitted to the Galvanic action during 96 hours in all.

The following effects were perceived in this interval. The surface of the solution of muriate of soda became frothy. A kind of whitish pellicle covered, in a great measure, the inner sides of the tube, and gradually trickled down to the junction of the glass and its supporter or stalk of tin. The solution became slightly turbid.

In the pure distilled water the effects were more remarkable. After some hours of Galvanic action the surface of the water assumed a feeble shade of greenish yellow. This colour gradually spread to the lower parts. The gold wire inserted into it seemed to be attacked.

The piles were dismounted, and the apparatus remained under the the bell glass eight days longer; during this time the pure distilled water had entirely changed its state. The intensity of the greenish yellow was increased. This colour extended into the corresponding branch of the syphon, into its bend, and even into a part of the second branch. The gold wire inserted in this side was so much dissolved as to be perceptible by the naked eye; and two days after the action of the pile had ceased, the part of this wire that had been inserted, being about 65 millimetres long, had totally disappeared.

The tube containing the solution of muriate of soda remained in the state already described; only the semi-opake pellicle of the sides was raised into the lower part of the correspondent branch of the syphon.

In this state the bell glass and the syphon having been taken away, a strong smell of oxymuriatic acid was manifested in the bell glass, the syphon, and, above all, in the distilled water. Some drops of this water discoloured the tincture of turnsole. Tried with the solution of muriate of

silver,

silver, this same water immediately yielded a very abundant and very thick precipitate of muriate of silver. A piece of tin foil plunged in this liquid assumed the deep purple of the precipitate of Cassius.

Upon decanting this liquor, we observed at the bottom of the tube a residue which was at first white, but which on being exposed to the air speedily assumed a dark hue, and which was muriate of silver resulting from the solution of the alloy contained in the gold wire which was dissolved by the muriatic acid.

The tube containing the solution of muriate of soda even emitted a very penetrating smell of soda. The gold wire inserted into it was covered with a dark brown oxide, not very adhesive. The liquor of this tube greened the tincture of violets; with the muriate of lime it produced a milky precipitate. The same trials made with the simple solution of muriate of soda neither altered the violet tincture nor troubled the transparency of the muriate of lime.

The decomposition of the muriate of soda had not been complete in this experiment; the taste of this muriate was also very perceptible.

The last fact observed was, that the whitish pellicle, which almost entirely covered the interior sides of the tube containing the solution of muriate of soda, was not extended to the lower part fastened in the stalk of tin which served as a support; so that it seems the light had some influence upon this effect.

It results from this experiment, that with the apparatus employed by the Galvanic Society we may obtain at once, easily, and without giving reason for any doubt, oxymuriatic acid diluted in distilled water, and that we also obtain the separation of the soda from the muriate of soda.

As to the manner in which these chemical phænomena operate, or as to the causes which transform distilled water into oxymuriatic acid, without the solution of muriate of soda retaining in its decomposition any apparent trace of the presence of this acid, the Galvanic Society has not as yet hazarded an opinion. They have confined themselves to varying the experiments, in order, if possible, to throw new light

light upon the theory; and they will consider it their duty to lay their results before the public if they obtain any that are worthy of attention.

I repeated this experiment, changing the poles and substituting platina wires in place of the gold ones. It was then in the solution of muriate of soda corresponding with the positive pole that I obtained the muriatic acid. The distilled water communicating with the negative pole greened the tincture of violets very strongly. It had contracted a slight taste, and was a little turbid: the salted water in the tube on the positive side preserved all its transparency.

I made the same experiment with two tubes, both of them filled with a solution of muriate of soda. In this experiment, as in the two others, it was also the positive pole that gave the muriatic acid, and the negative pole that manifested the alkaline characters. In this last tube the solution was slightly turbid. The two solutions feebly oxidated the platina wires. The gas liberated on the negative pole is always most abundant, even when this side contains pure water.

These experiments were continued several days, with a pile of 15 pairs only, of zinc and copper; this was sufficient in order that the effects should be manifested very strongly, as well by the smell and taste of the liquids, as by their action upon the vegetable tinctures.

M. Riffault, at whose house the committee of the Galvanic Society met, repeated the experiment made by the committee, of which he has given the preceding account; substituting, however, the nitrate in place of the muriate of soda in the tube communicating with the negative pole. He also soaked in a solution of nitrate of soda the pieces of cloth employed in the composition of his piles. The Galvanic effect was very feeble in this apparatus; it was only sensible at the positive pole; *i. e.* in the tube of distilled water. No bubble appeared upon the gold wire in contact with the negative pole which entered into the solution of nitrate of soda.

The solution of muriate of soda was then substituted for that of nitrate of soda, but only used for the cloth of the piles. This change produced more effect, but still in the

distilled

distilled water, or on the side of the positive pole, without any disengagement of gas having been perceived in the solution of nitrate of soda, or from the negative pole.

But the poles having been changed, so as to make the distilled water communicate with the negative pole, and the solution of nitrate of soda with the positive pole, the energy of the pile became instantly very sensible in the two tubes; and there were soon established, towards both poles, considerable currents of bubbles, and continuing nearly 200 hours quite visible through the bell glass which covered the apparatus.

M. Riffault has also subjected to experiment the solution of nitrate of soda in both tubes. He still continues these two experiments, and attends to them with great care. He hopes they will present some interesting results, with which he will make the Galvanic Society acquainted.

VIII. *On a new Mode of equally Tempering the Musical Scale. By Mr.* JOHN FAREY.

To Mr. Tilloch.

SIR,

FOR the information of such of your musical readers as may be advocates for the *isotonic* or *equal temperament* scale, and also for enabling those who prefer other systems, to make deliberate and just comparisons of that system in practice, with their own favourite systems, I embrace the present opportunity of stating, that in pursuing the inquiry which (vol. xxvii. p. 204, of your Magazine,) I recommended to musicians, and beg here still further to recommend, into the many different *wolves* which can arise by comparing the seven conchords with each other, in all their possible combinations, I discovered, on the 12th ult.—that 5 true *fourths*, when compared with 3 true *fifths* and a true major *third*, or 54ths — 3 V — III, give a wolf or difference, which agrees with the proper flattening of the *fifths* in *equal temperament*, to a surprising degree of exactness; differing therefrom no more than 3,2114 in the logarithms of eleven places

 of

of figures, wherein the elementary *comma* ($\frac{81}{80}$) is expressed by 53950,3189; being an error but of *one* part in *sixteen thousand eight hundred* parts of a *comma!*

The practical application of this will consist, in tuning five successive *fourths* upwards from the key or pitched note, each *quite perfect*; from the last note tuning downwards two *fifths*, and a major *third* in succession; and considering the last note so tuned, as the equal temperament *fifth*; from which 5 4ths are again to be tuned upwards and 2 V + III downward, for the second Vth in this scale, repeating the operation as above, and descending octaves when necessary, until the twelfth Vth is compared with the octave of the key-note, from which it will not differ in any sensible degree, if the operation be rightly performed; because 12 of my fifths as above*, differ from 7 octaves but $\frac{1}{1400}$dth part of a comma.

I intend to publish the further details of this system when leisure will permit; and am, sir,

Your obedient servant,
JOHN FAREY.

12, Upper Crown-street, Westminster,
June 21, 1807.

IX. *Description of the Optigraph (invented by the late Mr.* RAMSDEN) *as improved and made by Mr.* THOMAS JONES, *Mathematical, Optical, and Philosophical Instrument Maker, No.* 124, *Mount-street, Berkley-square.*

THE methods used to facilitate the practice of drawing in perspective, as well for those versed in this polite art, as for those who have made less proficiency, have been various and numerous. Though some have supposed that the warmth of imagination and luxuriance of fancy, which impel the mind to the cultivation of the fine arts, are not to be confined to mechanical modes, yet upon observation and inquiry they will find that the most able and accomplished artists are often obliged to have recourse to some rules, and to use some mechanical modes to guide and correct their

* Which are each 2t + 3H, or $\frac{3^7 5}{2^{14}}$, and their *log.* ·8243988,5.

pencil:

pencil: but so tedious is the operation, and great the difficulty, of representing objects in true perspective, that they trust mostly to their eye and experience for success. The result of such a mode of proceeding may be determined by portraits drawn by the best artists, and the different judgments formed concerning them. It has been well observed, that there is no artist who will be hardy enough to say that he can delineate by the eye the same object twice with exactness, and preserve a just and similar proportion of parts in each. In one of the figures we shall find some of the parts larger than in the other; both cannot be right: yet supposing them perfectly the same, neither may be conformable to nature. In addition to this, many situations of an object occur, which no eye, however habituated, can represent with accuracy.

On this account many attempts and various instruments have been made for the purpose of giving the outline of an object with accuracy.

The late most ingenious Mr. Ramsden, so well known for his inventions and improvements in various instruments, considered the present subject an object worthy of his attention, and invented the instrument I am about to describe, which is so simple and easy in its operation, that a person not possessed of the least knowledge of drawing, may, with less than three minutes' instruction, be perfectly able to take a perspective view of landscape, building, machinery, or, in fact, an object of any description presented to his eye, with the utmost correctness.

Mr. Ramsden left this instrument without the means of enabling the operator to enlarge or diminish his drawing; an inconvenience which I have obviated, while at the same time I have added some other trifling improvements. This instrument is certainly superior to any hitherto constructed for the same purpose; for in this the operator views the object through a telescope, which enables him to delineate minute objects with great exactness and ease, which are often too far from the eye to be seen sufficiently well to be delineated correctly.

 Fig. 1.

Fig. 1. (Plate II.) is a perspective view of the optigraph. A represents the drawing board, on the outside frame of which is fixed the pillar of the instrument, B, by a clamp *a*. C is a tube (sliding in the pillar), on the top of which is fixed, by means of a screw *c*, the frame D; at the end of this frame is a plain mirror E, beneath which is suspended, by a universal joint, the telescope F, of which G is the eye-tube. H are sliding tubes, capable of being shortened or lengthened in the same proportion as the inside speculum *c*, (fig. 2) which is fixed to any place by the clamp screw E. The pencil L, of which *h* is the handle, slides perfectly easy, without shake, in the tubes H: the pencil is so contrived as to have all the freedom of a pen when held in the hand for use.

Fig. 2. represents a section of the telescope, being the principal part of the invention. The rays from an object entering the plane mirror *a*, are reflected into the telescope, passing through the object-glass *b*, and entering the speculum *c*, are reflected through the eye-glass *d*, to the eye at *e*: *f* is a piece of parallel glass, with a small dot on its centre, exactly in the focus of the eye-glass *d*.

Mode of using the Optigraph.

Fix the drawing board to the table (by a clamp which is packed in the box) so that the surface of the mirror E is nearly parallel to the object; then take hold of the handle *h*, and hold the pencil on that part of the paper where you would wish the centre of your drawing, or any part thereof, to be. Then place your eye at the eye-tube G, and with your left hand alter the inclination of the mirror E until the small dot, described at *f*, in fig. 2., is on some particular part of the object that you wish to begin with, adjusting the telescope to distinct vision by the milled head P. Then by moving your hand (having the pencil) you pass the dot seen in the field of the telescope over the object, the pencil marking it at the same time on the paper.

To make your drawing larger, pull out the tube of the pillar C, fig. 1, and fix it with the screw *e*; then pull out the

the sliding tubes H, till the pencil is within half an inch of the paper (in the middle of the board), and proceed as before.

To make the drawing smaller, shorten the tubes C and H by sliding them in, and proceed as before.

Any thing not clearly understood will be explained by the maker.

The price of the instrument is five guineas, neatly packed in a box fourteen inches long, three inches deep, and six inches wide.

X. *History of Astronomy for the Year* 1806. *By* JEROME DE LALANDE *.

THE comet discovered by M. Pons at Marseilles deserves to be first mentioned in the History of Astronomy for 1806: he saw it on the 11th of November, in the morning, and it is the 97th we are acquainted with, according to the catalogue given in my *Astronomie*, and in the different volumes of the *Connoissances des Tems* published since the year 1792, in which I have given notices of the comets; and it is the sixth which M. Pons found since the 11th of July 1801. M. Thulis, director of the observatory, observed it as well as the bad weather would permit him, as the months of November and December are inimical to astronomical pursuits even at Marseilles. This comet was very small, shapeless, without any sensible nucleus, and not visible to the naked eye. On the 9th of November, at 17^h mean time, it had $181° 3'$ right ascension and $2°$ of northern declination.

As soon as I received this intelligence I communicated it to M. Bouvard and M. Burckhardt, who observed it at Paris; the latter furnished us, on the day following, with the elements of its orbit, and continued to observe it: on the 18th of December, at $16^h 26'$ mean time, it had $11^h 12' 41''$ of right ascension and $32° 57'$ of declination: it advanced

* From *Magazin Encyclopédique*, February 1807, p. 364.

 rapidly

rapidly to the south. It was not again seen until the 25th of January.

The following were the elements calculated on the 29th of January: inclination, 35° 6′; nodes, 10^s 22° 13′; perihelion, 3^s 6° 57′; passage, 28th of December, at 20 hours, movement retrograde: perihelion distance 1·080. It was then visible in the evening, having returned at 29° of south declination, and at 20° of right ascension; and they were able to calculate its elements with more precision.

I have spoken in my History of Astronomy for 1805, of the 95th comet, which was discovered on the 20th of October by Messrs. Bouvard and Pons, and which was observed to the 6th of November. M. Macaroil, of the Isle of France, wrote upon the 19th of December 1805 that he had observed a comet on the 14th towards the feet of Paon. M. Dupeloux observed it on the 13th, 14th, and 15th. The nucleus was 1′ in diameter, the atmosphere 45′: it was seen before the stars of the third magnitude; it was much more beautiful than when it was observed at Paris; and these observations, made at a time when it could not be seen in Europe, will be very useful in correcting its elements.

M. Burckhardt gave, in the fourth volume of the *Mécanique Céleste* of M. Delaplace, an explanation of the singular phænomenon of the comet of 1770, which the attraction of Jupiter rendered visible that year, having been previously invisible, and this attraction has rendered it invisible in future. The comet of 1762, according to M. Burckhardt, only agreed at 5′, but the correction of refraction went the length of 7′, and he rectified the elements in the following manner:

Nodes - - -	11^s	18°	33′	5″
Inclination - -	0	85	38	13
Perihelion - -	3	14	2	0

Transit 28th May, 8^h 11′.

Distance, 1·0090485.

M. Bessel has made some long researches respecting the comet of 1769; he found the period at about 2100 years: this results from the whole of Messier and Maskelyne's observations,

servations, which he has reduced with the most scrupulous exactitude. The errors in the calculation only extend to 5″, as well in right ascension as in declination. The great multitude of observations, and their exactitude, inclined him to think that the result was very probable.

Messrs. Gauss and Bessel have calculated the comet of December 1805, and that of 1772, in an ellipsis, and they have found such differences that it is difficult to suppose it to be the same comet, at least there must have been derangements.

M. Gauss has calculated that of 1805 in the parabola and ellipsis; and he has found that the whole ellipsis, the great axis of which exceeds 2·82, represents the observations better than the parabola. He thinks that there are probably plenty of comets where observations are not sufficient to prove that the orbits approach the parabola, and that it is necessary to calculate for each the limits between which the orbit is contained.

On the 21st of May a report was spread that the world would be at an end on the 25th; and the prediction was ascribed to me, of the comet that was to cause this awful event. I received several letters informing me that different persons were ill from terror, and that some had died. A man who hawked this prediction about the streets was arrested, and I disavowed it in the Paris journals. On the 25th a dreadful storm increased the general terror. On the 15th of January 1798 I was under the necessity of making a similar disavowal; several persons were sick, as in 1773.

There is in the history of comets a singular anecdote relating to two stars, marked A and S, in the Memoirs of the French Academy for 1775: the comet had been discovered near these stars on the 8th of August 1769. The letters refer to two natural daughters of the count de Charolais, *Adelaide* and *Sophia*, whom M. Bouret wished to have occasion to mention when the king was at his house, where M. Messier presented his chart to his majesty. There is one of these stars in the grand atlas of M. Bode, but without any letter attached to it. I induced him to insert

in this atlas 800 stars which M. Messier had occasion to determine, which are in the various volumes of the Academy where there are any details of his observations upon comets; but several of them have not yet been published.

The Transactions of the Royal Society of London for 1804 contain experiments upon the measurement of small angles, and upon the size of Harding's planet, by Mr. Herschel: he finds a fourth of a second for it, but does not decide whether it is a real diameter or not.

M. Pigot gives the changes of the star of the fifth magnitude in Sobieski's Buckler from 61 and $\frac{1}{2}$ to 62 and $\frac{3}{4}$ days, which is sometimes scarcely visible. He discovered it in 1795: its position was in right ascension 279° $9\frac{1}{2}'$, declination 5° 56′ A, June 1796: its smallest lustre 1796, 17th of September and 13th of November; 1797, 14th of May and 7th of August; 1798, 29th of July and 15th of September; 1799, 7th of August and 11th of October; 1800, 14th of July and 24th of September; 1801, 9th of August. Ninth magnitude, or invisible.

Part of these observations were made at Fontainebleau in 1803, before the National Institute had procured M. Pigot liberty to return to England.

Mr. Herschel examines the effect which the displacing of the solar system should produce: he reduces to 1·5″ the proper annual movement of six principal stars, supposing that the sun was directed towards 245° 52′ of right ascension and 49° 38′ of declination. Maskelyne had $5\frac{1}{4}''$ for the sum of the six annual movements of these six stars; the surplus is the effect of the displacement of the sun.

Mr. Herschel has given some observations upon the singular figure of Saturn. On the 12th of April 1805, with a 7-foot telescope which gave an ordinary distinctness, and which magnified 570 times, he found the ring whiter, and Saturn yellowish.

With a 10-foot telescope, which magnified 527 times, he found the four points of the greatest curvature at 43°; he compares it to a parallelogram, the four corners of which are rounded.

With

With a 40-foot telescope, magnifying 360 times, he saw the same. The axis is 32°, the equator 35°; the diameter of the greatest curvature 60°.

In this he finds the effect of gravitation upon the figure of the planets: there are here two centripetal and two centrifugal powers, since he has proved the two rotations of the planet and of the ring.

The most distant ring turns sensibly. The division between the rings is obscure, as is the space between them.

The publication of the second volume of Bradley's Observations for 1756, and subsequent years, has been an interesting object to us. Those of his successor are added, and this volume joins those of Mr. Maskelyne, which begin in 1765.

Among the curious observations of this year we may reckon that of the *annual parallax of the stars*, which M. Calandrelli thought he recognised in several stars. M. Piazzi had given some results upon this subject, which I have mentioned in the history of the former year.

The *parallax of the Lyre in declination* is 0·875 of the absolute parallax; it passes nearly enough the zenith. M. Piazzi has observed it a great deal. But we are almost tempted to draw from the observations of M. Piazzi a consequence contrary to that which he wishes to establish; it is, that the parallax of the stars still escapes our measurements: if it was large enough to be measured, the declination would not be always the surest method to employ. To conclude: M. Piazzi proposes to continue his researches; and M. Calandrelli, at Rome, has given two memoirs upon the parallax of the Lyre.

Roma, 1806, in 8vo. *Rissultato di varie Osservazioni sopra la Parallase annua di Wega.* The parallax is found at 5″ with a 9-foot sector. The observations agree well enough to encourage him to think that this parallax really exists; which has not been hitherto believed.

We received in the month of December some *Opuscoli astronomici*, printed at Rome by Messrs. Calandrelli and Conti, twenty pages in quarto, wherein there are six memoirs,

moirs, one of which is upon the annual parallax of the Lyre, which he finds as at 4·4″; another upon the *opposition to the planet Herschel* 1805, and a third upon the elements of its orbit. The rest are *upon the eclipse of the Sun in* 1806, observed at Rome and Padua, and calculated by M. Conti; upon the method of corresponding altitudes; and, finally, a nonagesimal table for the latitude of Rome.

The parallax of 4·4″ would reduce the distance of the Lyre to 1600 millions of leagues in place of 7000; but as the parallax of right ascension of the Lyre varies much more than the parallax of declination, it is to be wished that this method were employed for clearing up this curious question.

The greatest parallax of the Lyre in right ascension is between the end of May and the end of September; in declination, between the end of June and the end of December; and as it was towards the month of August that it was most observed passing the meridian at eight o'clock, it is possible that the difference may have escaped the most exact observers.

The prolongation of our meridian, undertaken this year, cannot fail to be interesting to astronomers. We therefore think it our duty to apprise them in what state it is, and to assure them of the certainty soon completely succeeding.

Since the 2d of May M. de Laplace proposed to continue the meridian to the Balearic islands. Messrs. Biot, Arrago, and Rodriguez the Spanish philosopher, set out with instruments on the 2d of September.

During M. Arrago's absence his place is filled in the observatory by M. Claude Louis Mathieu, born at Macon in November 1791, who is well skilled in astronomical observations and calculations.

As they were also to determine the pendulum at 45°, on the 26th of July Messrs. Bouvard and Biot put in trial the invariable pendulum of platina intended to be carried into the different points of the meridian. In order to deduce the simple pendulum from it, and the variations in gravity, they made it oscillate before the pendulum of a clock, the pace of which was perfectly well known, and they observed from a distance,

a distance, through a glass, the coincidence of the two pendulums; there was not a minute of uncertainty upon the time at which they exactly agreed.

So early as 1775 M. Turgot, then minister, wished to send M. Messier to Bourdeaux, in order to have the pendulum at 45°. His retiring from public life prevented the success of this enterprise, but at present we have much more perfect methods.

M. Biot writes from Barcelona on the 22d of September, that he was received in the handsomest manner by the heads of the Spanish government. Upon the fourth of October, he writes from Tarragosa, that the grand triangle will be easily made between the middle of November and the end of February.

Upon the 12th of October he set out for Valentia, and then proceeded to Cullera, where he reckons upon one station; the rains are still an obstacle; but in the month of November the north winds will clear the sky.

On the 15th of October, the small advice boat or brigantine, *Le Mystique*, which was to carry the astronomers to the island of Ivica, arrived; it goes with sails and oars, and is commanded by a very experienced and very zealous officer, M. de Vacaro: it is not armed. The passports of the English admiralty arrived. They were to embark at Denia, 15 leagues to the south of Valentia; from Denia to Ivica is only 25 leagues, and this great triangle would be easily made in the months of January and February.

23d October, M. Biot embarked for Ivica; he returned on the 10th November. The result of this voyage was, that they were to choose the port of Mongon, near Denia, in place of Cullera, and the mountain of Camrey in the island of Ivica. All the triangles will be finished in two months; but the latitudes will not be observed until the end of the year at Formenteva, a small island in the neighbourhood of Ivica, where a base will be measured. M. Chaix has taken charge of the post of Mongon, M. de Vacaro superintends another station.

M. de Vandeuil of Madrid, M. Viot at Barcelona, M. la Nusse at Valence, and M. Morand at Denia, took great interest

terest in forwarding the preparations, and furnished all the requisite assistance. Infinite pains were taken in preparing the stations; 200 men were employed in cutting a sentry box in the rock of Mongo; 60 men, and as many mules, at Ivica: the tents were blown down by a dreadful north wind; but at last, on the 7th of December, all the reverberators and signals were placed. M. Arrago is indefatigable. Thus the sciences have lost nothing by the war. The Royal Society of London requested the French Institute to set at liberty an English astronomer, who was a prisoner in France; which was instantly complied with: this is the second time the sciences have had this privilege granted them.

On the 11th March, 1806, the council of state decided upon giving captain Flinders his liberty, and upon restoring his galley the Cumberland.

The sixth volume of the Memoirs of the Institute for the class of Sciences contains the eulogy upon M. Mechain; a new formula to reduce distances by several observations, by Messrs. Mechain, Messier, Burckhardt, and Lalande (my nephew). I have there given the results of the transit of Mercury in 1802, and the consequences resulting from it, in order to correct my tables. M. Coulomb has there given "Methods for obtaining the greatest degree of magnetism," and M. Buache a memoir upon the discovery of America; where he shows that Columbus had consulted Toscanelli, and that the Isles Antillia, which had been thought to be America, were the Azores, which he recognised in a chart of 1367: he thence concludes that it was certainly Christopher Columbus who discovered America. But I have shown in my Abridgement of Navigation, that there were several preceding events which led him to it.

The class of Sciences has also published the first volume of *Savans Etrangers*, which has been a long time expected: it contains some observations of M. Bouvard; some memoirs of M. Burckhardt upon *several comets and upon micrometers*; a memoir of M. Dangos upon the terrestrial refractions; meteorological observations made at Cayenne during 10 years, by M. Mentelle, whose loss we regret: he

was

was brother to our celebrated geographer, and I had procured him the means of going to exercise his zeal in a country almost unknown, where important observations might be made: to him only we are indebted for any knowledge of the marshes of Cayenne.

The class of Sciences having resolved to publish their memoirs half-yearly, were only able to publish their volume for the first half of 1806 on the 8th of December. It contains memoirs by M. Legendre upon triangles, in the spheroids; by M. Burckhardt upon the comets of 1762, 1770, and 1784, and upon Harding's planet; and some experiments of Messrs. Biot and Arrago upon the refractive power of the different gases, a very interesting object to astronomical refraction. The class of Sciences offers a considerable prize for an account of the perturbations of Olbers's planet.

M. Delambre has observed the equinoxes and the solstices, and has found no cause for changing his new tables.

M. Piazzi has examined the question on the obliquity of the ecliptic, which has been a long time in dispute. The winter and summer solstices of 1804 have given him 7″ less in winter than in summer: he does not think that this can arise from the variations in refraction: he was led to ascribe it to the light of the sun being more refrangible than that of the stars, whence it would follow that the refractions deduced almost singly from the stars would not agree with the sun: he thinks he is certain, from his own observations, that the refractions of the stars are the same night and day. He thinks that from the zenith to the winter solstice the refractions can only be two seconds in error: this is not sufficient to explain the differences from 7″ to 8″ found between the two solstices by the observations of Messrs. Piazzi and those of Greenwich. M. Delambre only finds 4″ as well as M. Chiminello. M. Piazzi thinks that a new element must enter into the calculations of refractions, expecting further light upon the subject: he thinks that the obliquity cannot be deduced except from the observations of

summer.

summer, and he finds 23° 27′ 57″,4 for 1800, which only differs by 0″,3 from what M. Delambre found by his 12 solstices, making a slight increase to the refractions of Bradley, and diminishing by one second the latitude of Paris, which will be 48° 50′ 13″. M. de Laplace supposes that the difference between winter and summer proceeds from our taking the interior thermometer in place of the exterior thermometer, which in his opinion ought to have given the true measure of refraction.

These four useful collections of observations, the Ephemerides of Berlin, of Vienna, of Milan, and the Connoissance des Tems, have been continued.

England does not afford us the same assistance: it seems no observations are made any where except at Greenwich, and the three kingdoms all depend on Dr. Maskelyne: but it must be confessed that the observations he publishes every year are worthy of compensating for every deficiency.

The Connoissance des Tems for 1808, which made its appearance on the 15th September, 1806, by the cares of M. Delambre, begins a new collection of these useful ephemerides. The volume is thicker, the subjects it embraces more extensive, and the calendar has 15 months, in order to form a continuation to the volume for the year 15: we there find all the observations made by M. Bouvard in the year 12, 1803 and 1804, with the fine instruments belonging to the Imperial observatory.

M. Delambre has given in detail the measure of the degree in Sweden; I shall here give the result, which is 57200 toises in place of 57422 which the French found in 1736; and I shall add a reflection which seems to be useful to explain this difference. They had not at that time the means of verifying the parallelism of the glasses, as I did not describe the proving glass until 1764, when I published the first edition of my Astronomy.

I have given in this volume a table of the proper movements for 500 stars, calculations for the eclipses for 1805 and 1806, and conjunctions of Venus.

M. Delambre has there given a history of astronomy; notices

notices of several useful books; formulæ of various authors; corrections of the solar and lunar tables; and a method to find the configurations of the satellites.

We there find observations of Messrs. Vidal, Flauguergues, Scarpellini, Humboldt, and a memoir of M. de Prony upon the calculation of the spheroids, and the elements of the three last comets.

[To be continued.]

XI. *Notices respecting New Books.*

Philosophical Transactions of the Royal Society of London, for the Year 1807. *Part* I.

This part contains the following papers:—1. The Bakerian Lecture, on some Chemical Agencies of Electricity. By Humphry Davy, Esq. F.R.S. M.R.I.A.—II. On the Precession of the Equinoxes. By the Rev. Abram Robertson, M.A. F.R.S., Savilian Professor of Geometry in the University of Oxford.—III. An Account of two Children born with Cataracts in their Eyes, to show that their Sight was obscured in very different Degrees; with Experiments to determine the proportional Knowledge of Objects acquired by them immediately after the Cataracts were removed. By Everard Home, Esq. F.R.S.—IV. Observations on the Structure of the different Cavities which constitute the Stomach of the Whale, compared with those of ruminating Animals, with a View to ascertain the Situation of the digestive Organ. By Everard Home, Esq. F.R.S.—V. On the Formation of the Bark of Trees. In a Letter from T. A. Knight, Esq. F.R.S. to the Right Honourable Sir Joseph Banks, Bart. K.B. P.R.S. &c.—VI. An Investigation of the general Term of an important Series in the inverse Method of finite Differences. By the Rev. John Brinkley, D.D. F.R.S., and Andrews Professor of Astronomy in the University of Dublin.

Appendix.—Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

The

The Artist: a Series of Letters on Science and Art. Edited by Prince Hoare. *4to.*

It has long been a matter of regret to those who wish well to the arts in general, and to the fine arts in particular, and who can properly appreciate their importance, and the rank they ought to hold in civilized society, that means were not sooner adopted to diffuse a knowledge of them more generally in this country. But, in truth, it was more easy to see the defect than to apply a remedy. Artists themselves were either too little impressed with this truth, that in the fine arts, as in every science, the public mind must be tutored and instructed before it can be so captivated as to seek to cultivate any correct acquaintance with them; or they were too diffident of their own powers to believe that any beneficial effect could result from their individual exertions in the field of literature. Some, indeed, who are called artists, but who are unworthy of the name, feel so little for the dignity of their profession, that they hesitate not to sacrifice to false taste for the sake of gain, and thus contribute all in their power to propagate ignorance and absurdity. With these we mean not to occupy our readers' time; but to those who feel the captivating powers of art, and who are impressed with the importance of the instruction and information which fine productions are calculated to afford to the human mind, we would humbly suggest that they have no more right to be angry with the public for not relishing them, than they have to be angry with any person who refuses to eat when he is not hungry. Let the appetite be once excited, and entreaties will then be unnecessary to induce the public mind to satisfy its own cravings. To produce this appetite, a better mean could hardly be devised than such a publication as the present, if the contributors will only keep in their recollection, that, in a great measure, they have yet to beget that taste in others which affords to themselves such high gratification, and that every endeavour to diffuse knowledge requires much patience, benevolence, and even a considerable portion of self-denial. Their chief difficulty will be to command readers, for people do not willingly sit down to an entertainment composed of dishes to which

which they are entire strangers: they ought therefore to blend their instructions in matters relating to the fine arts, with subjects to which their readers may not be entirely strangers; and these, too, must be distinguished by taste; for in vain will they look for disciples, except among those who have made progress in other arts and sciences.

This work, which is published in weekly numbers, of which sixteen have already appeared, is "written by men of eminent professional abilities, on topics relative to their respective studies, and by other persons peculiarly conversant with those subjects;" and each essay has the signature or initials of its author. Among the number of contributors we observe West, Flaxman, Northcote, Hoppner, Hope, Cumberland, Cavallo, Boaden, Soane, Shee, Hoare, and Mrs. Inchbald. After mentioning such names, need we add that "The Artist" deserves, and we hope will obtain, the attention of the enlightened part of the community? The aim of the contributors to this work is so highly meritorious, that it would be worse than cruel to dwell on any little inaccuracies in the style of a few of the papers, especially when we can add, with truth, that there is hardly a single page in the whole work that does not convey both amusement and instruction.

No. VII. of the work is entirely dedicated "to the memory of John Opie;" a tribute grateful to every friend of merit, a part of which we shall therefore select as a specimen of the work before us.

"Few men have attained to eminence by a more irregular course of study, by stronger native endowments, or by more determined industry, than the great painter whose name at this moment inspires public regret. The child of humble life, born in a remote and secluded part of the island, with little or inferior education, such as humble and busy parents could bestow, he was destined to transplant to the bosom of the metropolis the hardy products of a sound and vigorous intellect, and to add strength and lustre to civil cultivation.

"John Opie was born in May, 1761, in the parish of St. Agnes, about seven miles from the town of Truro. His father and grandfather were reputable master carpenters in

 that

that neighbourhood. His mother was descended from the antient and respectable family of Tonkin, of Trevawnance, in Cornwall, and, amongst his ancestors in that line, is mentioned the author of a valuable history of Cornwall, which was left nearly finished, and is at present in the possession of lord De Dunstanville.

"He was very early remarkable for the strength of his understanding, and for the rapidity with which he acquired all the learning that a village school could afford him. When ten years old, he was not only able to solve many difficult problems of Euclid, but was thought capable of instructing others: and such was his increasing confidence in his own superior powers, that he had scarcely reached his twelfth year, when he set up an evening school in St. Agnes, and taught arithmetic and writing, for the latter of which he was excellently qualified, as he wrote many various hands with admirable ease and accuracy; and he reckoned among his pupils some who were nearly twice his own age.

"His father was very solicitous to bring him up in his own business, and to this end bound him apprentice to himself; but the soaring mind of the boy could not submit itself to drudge in the employment of a common man. The love of drawing and painting seems to have given a very early bias to his inclinations; and the manner in which it disclosed itself cannot be considered as uninteresting.

"Emulation appears to have first lighted up the ready flame. About the tenth year of his age, seeing one of his companions, whose name was Mark Oates (now a captain in the marine service) engaged in drawing a butterfly, he looked eagerly, in silence, at the performance: on being asked what he was thinking of, he replied, "he was thinking that he could draw a butterfly, if he was to try, as well as Mark Oates." He accordingly made the experiment, and triumphed; and he returned home to his father's house in high spirits, on account of the victory he had obtained.

"From this moment the bent of his talents was determined. It happened soon afterwards that his father, being employed in the repairs of a gentleman's house in Truro, young Opie attended him: in the parlour hung a picture of a *farm-yard*,

a *farm-yard*, probably of humble execution, but of sufficient merit to attract his notice; and he took every opportunity of stealing from his father's side to contemplate the beauties of this performance, which, in his eye, were of the highest class. His father, catching him in one of these secret visits, corrected him: but this had little effect; he was soon again at the door of the parlour, where being seen by the mistress of the house, he was, by her interference, permitted to view the picture without interruption. On his return home in the evening, his first care was to procure canvass and colours, and he immediately began to paint a resemblance of the farm-yard. The next day he returned to the house, and again in the evening resumed his task at home. In this manner, in the course of a few days, by the force of memory only, he transmitted to his own canvass a very tolerable copy of the picture.

"Nearly by the same methods he copied a picture of several figures hunting, which he saw in the window of a house-painter. In his copy, however, he had, in compliance with the *costume* of his neighbourhood, placed a *huntress* upon a pad instead of a side-saddle; and being laughed at for this mistake, he some time afterwards destroyed his copy.

"The love of painting had thus so thoroughly established its dominion over his whole mind, that nothing could now divert him from engaging in it as a profession: his father, however, still treated his attempts with great severity, and used his utmost endeavours to check a pursuit, which he considered as likely to prove injurious to his son's future prosperity: but the aspiring views of the young artist met with a zealous supporter in another part of his family: his father's brother, a man of strong understanding, and moreover an excellent arithmetician, continued to view his progress with pleasure, and encouraged him in his desire of learning, by jocularly complimenting him with the name of *the little Sir Isaac*, in consideration of the knowledge he displayed in mathematics.

"He therefore followed his new studies with ardour, and had already attained a competent skill in portrait painting, and had hung his father's house with the pictures of his

family, and his youthful companions, when he became accidentally known to Dr. Walcott, then residing at Truro, (and since so celebrated under the title of *Peter Pindar*,) who having himself some skill in painting, a sound judgment, and a few tolerable pictures, was well fitted to afford instruction, and various advantages, to the young scholar.

"Thus assisted and recommended, his fame found its way through the country; and so rapid was his progress, that he now commenced professed portrait-painter, and went to many of the neighbouring towns, with letters of introduction to all the considerable families resident in them.

"One of these expeditions was to Padstow, whither he set forward, dressed, as usual, in a boy's plain short jacket, and carrying with him all proper apparatus for portrait painting. Here, amongst others, he painted the whole household of the ancient and respectable family of *Prideaux*; even to the dogs and cats of the family. He remained so long absent from home, that some uneasiness began to arise on his account; but it was dissipated by his returning dressed in a handsome coat, with very long skirts, laced ruffles, and silk stockings. On seeing his mother, he ran to her, and, taking out of his pocket twenty guineas, which he had earned by his pencil, he desired her to keep them; adding, that, in future, he should maintain himself.

"The first efforts of his pencil, though void of that grace which can only be derived from an intimate knowledge of the art, were true to nature, and in a style far superior to any thing in general produced by country artists. He painted at that time with smaller pencils, and finished more highly than he afterwards did when his hand had attained a broader and more masterly execution: but several of his early portraits would not have disgraced even the high name he has since attained. Towards the end of the year 1777, when he was sixteen years of age, he brought to Penryn a head he had painted of himself for the late lord Bateman, who was then at that place with his regiment (the Hereford militia), and who was an early patron of Mr. Opie, employing him to paint pictures of old men, beggars, &c. in subjects of which kind he was principally engaged, and which

which he treated with surprising force, and truth of representation.

"At length, still under the auspices of Dr. Walcott, he came to London, where his reception, and his continued progress, are the fit objects of the biographer. It is the purpose of this paper to delineate solely his *character*, as a man, a scholar, and an artist.

"Mr. Opie's ruling passion was ambition,—but ambition tending to the use and delight of mankind. It impelled him to eminence in his art, and it displayed itself in a resolution always decided, sometimes impetuous, to obtain every distinction which his path in life laid open to him. Accustomed in childhood to prove himself superior to his companions, the desire of competition became unextinguishable. Wherever eminence appeared, he felt and eagerly showed himself its rival. He was forward to claim the honours which he was still more diligent to deserve. He regarded every honourable acquisition as a victory, and expressed with openness the delight he experienced in success. On the professorship of painting in the Royal Academy becoming vacant by Mr. Barry's dismissal, he offered himself a candidate; and being told that he had a competitor, whose learning and talents pre-eminently entitled him to that office, he replied, that he abstained from further interference, but that the person who had been proposed was the only one in whose favour he would willingly resign his pretensions: consistently with his declaration, on Mr. Fuseli's appointment to the office of keeper, he renewed his claim, and was elected.

"Examples of a mind more open to the reception of knowledge, more undaunted by difficulty, more unwearied in attainment, are rarely to be found. Conducted to London by the hand of one who discerned his yet unveiled merit, he approached the centre of an exalted country with the liveliest hopes: he met its flatteries with trembling; and he viewed its unfeeling caprice with the sensitive emotions of genius, but with the unconquerable force of sense and judgment. An intellect naturally philosophic, soon discovered to him that he was not born to depend on the frivo-

lous conceit of crowds, but to command the respect of the great and wise. He bent his powers to the formation of his own mind: he applied himself to reading: he sought the society of the learned: ardent in his researches, boldly investigating truth, pertinacious (though not overbearing) in argument, while he elicited light from his opponent, and steady to principles which he found could not be shaken by controversy; in this manner, while an unremitting perseverance, superior to the neglect of the multitude, maintained the cunning of his hand, he became a scholar and a *painter*.

"The Life of Reynolds, published in Dr. Walcott's edition of Pilkington's Dictionary, was the first specimen of his literary ability. In this he displayed a profound knowledge of the subject, a quick and powerful perception of distinctive character, and a mastery of language little to be expected from a youth who was supposed to have been destitute of learning.

"He next published a letter in the Morning Chronicle, (since re-published in "An Inquiry into the requisite Cultivation of the Arts of Design in England,") in which he proposed a distinct plan for the formation of a *National* Gallery, tending at once to exalt the arts of his country, and immortalize its glories. To this he annexed his name, in consistence with the openness of character which at all times distinguished his actions.

"His lectures at the Royal Institution followed:—these were a spirited attempt to display the depths of his professional knowledge, amidst a circle assembled for entertainment and fashionable delight. His lectures impressed respect on his audience: they were full of instructive materials; they taught the principles of painting, and presented an accumulation of maxims founded on history and observation. But to whatever praise they might vindicate a claim, they never satisfied their author; and he declined the continuance of them. His election to the professorship of painting at the Royal Academy happening nearly at this time, he resolved to perfect what he had perceived defective; and he read at Somerset House four lectures, which, avoiding

ing any collision with the brilliant specimens of erudition and imagination which had immediately preceded him in that place, appeared to have been unequalled in their kind.

"In his former lectures at the Royal Institution, he was abrupt, crowded, and frequently unmethodical; rather rushing forward himself, than leading his auditors, to the subject. In the latter lectures he was more regular, progressive, distinct, instructive; and delivered a mixture of humorous and impassioned sentiment in a strain of clear, natural, and flowing eloquence. Here he found his genius roused, and his whole faculties adequately excited; and he shone more as professor at the Academy, than as lecturer at the Institution, because he was more formed by nature and application to address the studious and philosophic than the light and gay. He possessed no superficial graces, either in his conversation or professional practice. Every thing in him was manly, resolute, energetic; yielding little to fashion, nothing to caprice; less addressed even to fancy than to judgment; in no measure adapted to catch a careless glance, but fitted to awaken thought, and gratify reflection.

"It has been said by some, who most probably never exchanged a word with Mr. Opie, that his mind was without cultivation. That this was not the case, is plain from what has been related. It may not be amiss to notice, that Mr. Opie read French well, and understood something of Latin and music, all attained by his own unceasing application.

"It would be an omission of public duty not to add, that to whatever degree of respect Mr. Opie's talents finally raised him, he may yet be brought forward as another instance in which we have cause to regret the want of established public direction of his art. After the first flow of curiosity on his arrival in London had subsided, and when he could no longer be 'the wonder of the day,' 'the boy drawn out from a tin-mine in Cornwall,' his real qualities ceased to attract attention, and, what was worse, employment. His respectable and amiable patron, sir John St. Aubyn, stood his friend at that interesting

 moment;

moment; and, among many who might well have been proud to share the honour, he stood alone. But 'the progress of morals,' says lord Kaims, 'is slow; the progress of taste still slower.'

"The effects produced by hours of despondence on a mind so strongly gifted, who can measure? His intellectual strength, however, prevailed; the force of his endowments gradually, though slowly, raised him once more to admiration and to fame; the conscious sense of acknowledged merit re-animated his efforts; he exerted himself with perseverance, and rose to renown: he appeared to feel that he had just reached again the level of his self-opinion, when death extinguished his talents and his ambition.

"P. Hoare."

Mr. Opie expired on Thursday the 9th of April, 1807, and on Monday the 20th of the same month his remains were interred in St. Paul's Cathedral, near to those of sir Joshua Reynolds. The above sketch of Mr. Opie's life by Mr. Hoare is followed by a short but masterly account of his merits as a painter, which is generally understood to be the production of Mr. West; a short sketch of his character by Mr. Northcote; lines to his memory, and indeed worthy of his memory, by Mr. Shee—with other tributes from the pens of Mrs. Inchbald and Mr. Boaden.

A new edition of the Chemical Catechism is in the press, and will be published in the beginning of August next; much enlarged, and improved by the addition of a great number of new notes, and an account of all the new chemical discoveries. The vocabulary of chemical terms will also be found to be much improved, and the appendix will be enriched with a considerable number of new and interesting experiments.

XII. *Pro-*

XII. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

May 28. The right honourable Sir Joseph Banks, bart. president, in the chair.—A short paper was read on the magnitude and situation of Dr. Olbers's new planet called Vesta, whose orbit, it appears, is between Mars and Venus. The brightness and smallness of Vesta are such as to make it be considered a true asteroid.

A paper consisting of the description and use of a new eudiometer, invented by W. H. Pepys, esq. was also read. This paper was accompanied with a drawing of the apparatus, the chief novelty of which was the ingenious appropriation of two graduated glass tubes, to the ends of which are attached elastic gum bags, with other vessels, to convey the air under consideration into these graduated tubes. The importance of eudiometrical experiments to pneumatic chemistry is sufficiently known; and the advantage of an apparatus more accurate and manageable than that invented by Dr. Priestley and improved by Guyton, and also more generally applicable than that of Davy, must be very considerable. The great accuracy, however, and minute distinctions which the author (to whom the science of eudiometry owes much) has here proposed in the different methods of investigating the nature of the gases, would not be intelligible in the narrow limits of this report.

June 4. The President in the chair.—Dr. Herschel communicated his observations on the new planet Vesta, which differ little from those previously reported from the astronomer royal. This paper was accompanied by some curious details on the comet which appeared in the latter end of January and first of February last. This reappearance took place as the doctor had predicted from his observations, and has tended to convince him of the accuracy of his preceding calculation. Out of 16 comets with which he is acquainted, only two appear to possess a solid body or disk, and the other 14 seem composed only of luminous and perhaps inorganic

organic matter, which cannot be discriminated from their *comæ* or tails.

June 11. The president in the chair.—An interesting paper on the quantity of oxygen in carbonic acid, by Messrs. Pepys and Allen, occupied the attention of the society. These expert analysts preferred the use of charcoal to that of diamond for this purpose, as they conceived that very material errors existed in Guyton's experiments on this subject. They began with preparing charcoal from the different species of wood, by burning it in a platina tube, and, as usual, the woods yielded more or less charcoal according to their particular qualities and densities. This charcoal, when taken from the platina tubes and exposed to the atmosphere, absorbed water in quantities varying from 5 to 18 per cent., according to the nature of the wood from which it was made. The water of absorption was afterwards dispelled in vapour, without being decomposed, or giving out oxygen or hydrogen to the carbon. Carbonic acid was then prepared from this charcoal, which yielded to the acid, in different specimens, from 27·60 to 28·66 per cent. of carbon, the oxygen varying in the same proportions. Diamond was next examined, and it yielded 28·82 per cent. carbon, with an equal proportion of oxygen in the formation of carbonic acid. Hence it was concluded, that although the results of these experiments differ a little from the quantity of carbon supposed to be contained in carbonic acid, according to Mr. Tennant's experiments, yet it was evident that the diamond cannot be peculiarly an oxide of carbon, as has been alleged, since the oxygen consumed in the conversion of the carbon to acid was uniformly in proportion to the quantity of carbon employed. In the course of the experiments with the diamond, the authors conceive that no hydrogen was present, neither was there with the plumbago and dead coal, and the water absorbed by charcoal exposed to the air, was evaporated on exposure to a sufficient degree of temperature. To the accuracy of Guyton's experiments they particularly objected, and considered them as inconclusive and erroneous, especially in his use of nitrogen gas as a test for the oxygen.

June

June 18. The president in the chair.—A small quantity of the waters of the Dead Sea and the River Jordan, in Syria, which are known to be so noxious to animal life, having been brought to Sir J. Banks, Dr. Marcet procured about a pint, which he analysed, and laid the results before the society. From the smallness of the quantity (and the change the water must have experienced in the carriage) it was acknowledged that little confidence could be placed in such experiments. It appeared that these waters contain muriate of lime and magnesia in a very unusual quantity, besides sulphats, and other substances which could not be very accurately ascertained. The analysis adds nothing to our knowledge respecting the formation of asphaltum or bitumen in these waters.

After reading this paper, the Society adjourned, on account of the long vacation, till Thursday the 5th of November next.

SOCIETY OF ANTIQUARIES.

On the 11th, the right honourable the earl of Leicester, president, pronounced a feeling and well-merited eulogy on the very acute and profound talents of the late right reverend Dr. Douglas, bishop of Salisbury, many years a distinguished fellow and member of the council of the Society of Antiquaries. His lordship dwelt particularly on the national services of this most worthy and learned prelate in detecting falsehood, in vindicating the originality of English writers, and in laying down such clear, precise, and complete principles for separating historical facts, and inductive truth from designed fabrications or imaginary fables, as must in future either prevent literary fraud, or tend to detect it as soon as it may appear. Dr. Douglas was a Scots episcopalian, equally pure and rigid in his own moral œconomy as liberal and pious in his religious or political sentiments. His inquiry into the credibility of miracles, although not inaptly denominated the "Criterion," wants only to be divided into chapters and sections to be a much more useful investigation of the "Nature and Immutability of Truth," than the work which bears that name.

After his lordship had expressed his sentiments of the virtues

virtues and merits of this great man, the society proceeded to elect a new member of the council, as his successor, for the remainder of the season; when it appeared that Francis Annesley, Esq. was chosen for that office.

N. Carlisle, Esq. the secretary, furnished some learned and ingenious observations, by way of introduction to two excellent letters from the late bishop Warburton to Drs. Kennicot and Needham, relative to the similarity or identity of the Egyptian hieroglyphics and the Chinese characters, which are supposed to have been derived from the former.

TEYLERIAN SOCIETY AT HARLEM.

The above society, in pursuance of the intentions of its founder, has announced the following question:

"What is the reason that our school of painting at the period of its greatest splendour, and even at the present time, has furnished so few painters of historical subjects, while they have constantly excelled in every thing belonging to the imitation of nature, and in every thing which the narrow circle of domestic life presents to us? What are the best means for forming good historical painters in this country?"

The prize offered is 400 florins for the best answer to this question;—it must be transmitted before the 1st of April, 1808. The memoirs may be written in Latin, French, English, or German: they must be addressed to the House of Teyler's foundation, at Harlem.

XIII. *Intelligence and Miscellaneous Articles.*

MNEMONICA, OR NEW METHOD OF ASSISTING THE MEMORY.

In the Philosophical Magazine, vol. xxvi. page 282, we presented our readers with an article on this curious subject, translated from a German publication. The following particulars, relating to the same subject, are extracted from a letter

letter from a literary gentleman, at present in Paris, to his friend in London.

"Paris, 2d March 1807.

"During my residence in this metropolis I heard a great deal of a new method of *mnemonique*, or of a method to assist and fix our memory, invented by Gregor de Feinaigle. Notwithstanding the simplicity with which he announced his lectures in the papers, I could not determine myself to become a pupil of his, as I thought to find a quack or mountebank, and to be laughed at by my friends for having thrown away my cash in such a foolish manner. Perhaps I should hesitate to this moment about the utility of this new invented method to assist our natural memory, had I not had the pleasure of dining at his excellency's the Count of Metternich, the Austrian ambassador, who followed, with all his secretaries, the whole course of lectures: they all spoke very advantageously of it, likewise several other persons of the first rank I met there: in consequence of this I was inserted into the list of pupils, and I follow, at this moment, the lectures. All I can tell you about this method is: it is a very simple one, and easy to be learned, adapted to all ages and sexes: all difficulties in such sciences as require an extraordinary good memory, for instance, the names and epochs in history, are at once overcome and obviated. There is not one branch of science to which this method cannot be applied. It is easy to be perceived that such an invention cannot pass without some critique, and even sarcasms, in the public prints: some of them were very injurious, and plausible enough to mislead the public, who, knowing nothing of the method, are always more ready to condemn than to assist. Mr. Feinaigle, to answer all these critics at once, adopted a method not less public for Paris than the public papers, but less public for the rest of Europe: he gave, the 22d of last month, a public exhibition to about 2000 spectators, in which he did not appear at all, only about 12 or 15 of his pupils: each of them made such an application of the method as his situation in life required. The principal parts were the following: history about names and years; geography, with respect to longitude, latitude, number

number of inhabitants, square miles, &c. &c.; grammar in various languages, about different editions of the same work; pandects, their division, and title of each book, title, &c.; different systems of botany, poetry, arithmetic, &c. &c. At last one desired the company to give him one thousand words, without any connection whatsoever, and without numeric order; for instance, the word *astronomer*, for No. 62; *wood*, for No. 188; *lovely*, for No. 370; *dynasty*, for No. 23; *David*, for No. 90, &c. &c. till all the numbers were filled; and he repeated the whole (notwithstanding he heard these words, without order, and but once,) in the numerical order; or he told you what word was given against any one number, or what number any one word bore. It is still more striking, but certainly, likewise, more difficult, to retain as many numbers however great they may be. For words and numbers I could venture myself, with the greatest safety, as far as one hundred of each; and I am sure, after having fixed them once, which is done in less than ten minutes, I could repeat them to you at any period, without ever thinking any more of them.

"M. Feinaigle is about to visit England.

"I am, &c.

"LEOPOLD FICHTEL."

VACCINATION.

The instructions drawn up by the Vaccine Society of Denmark have been translated into the Icelandic language by M. Thorarsen. It is to be hoped that by this means the effects of the small-pox will be alleviated, which often made dreadful ravages in that country. The copies of the work have been distributed in Iceland, with appropriate engravings annexed.

There has been discovered at *Montefiascone*, in Italy, in a field adjoining the high road, a small cavern cut in the rock. The proprietor of the ground, having descended into it, found two dead bodies stretched upon a table of stone, apparently in good preservation, but which crumbled into dust as soon as they were touched: upon another table were placed

placed several vessels of earth and metal, which have been sent to the Pope, in order to be placed in the Museum of Antiquities of the Vatican Library: they are 21 in number.

A LIST OF PATENTS FOR NEW INVENTIONS.

To John Roebuck, of Warren Street, in the parish of St. Pancras, in the county of Middlesex, civil engineer; for his improvements in a machine called the Caledonian balance. May 14.

To Chester Gould, of Walworth, in the county of Surrey, gent.; for his improvements on a machine for mangling linen and other articles required to be mangled. May 26.

To Joseph Bowyer, of Kidderminster, in the county of Worcester, carpet manufacturer; for his method of working or manufacturing of carpeting for carpets and carpet-rugs, not heretofore used. May 29.

To John Brown, of the parish of Saint Andrew Hubbard, in the city of London, stationer; for certain improvements in the construction of a press for printing books and other articles, part of which may be applied to presses now in common use. June 2.

To John Bywater, of the town and county of the town of Nottingham; for certain improvements in the construction of windlasses for weighing the anchors of ships and navigable vessels, and various other purposes. June 6.

To Allan Pollock, of Paisley, North Britain, at present residing in London, merchant; for a stove of a new construction, and various improvements applicable to stoves, grates, and fire-places. June 11.

To Henry Maudslay, of Margaret Street, Cavendish Square, in the county of Middlesex, engineer; for improvements in the construction of steam engines. June 13.

To Francis Plowden, of Essex Street, Strand, in the county of Middlesex, esq.; for his safe and sure method of preserving for an extraordinary length of time, at sea and on land, butcher's meat, animal and other comestible substances, in a sweet, palatable and nutritious state, without acid, salt, or drying, the preservation of which aphthartic viands he conceives will be of great public utility. June 13.

METEORO-

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For June 1807.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
May 27	51°	64°	52°	29·95	41	Fair
28	52	66	47	·84	52	Fair
29	48	57	43	·68	30	Cloudy
30	43	45	46	·61	0	Rain, and remarkable high wind.
31	49	56	44	·82	15	Showery
June 1	47	58	49	·80	17	Showery
2	49	57	51	·92	20	Showery
3	46	57	46	30·00	21	Fair
4	54	60	53	·11	37	Fair
5	55	57	52	·07	30	Cloudy
6	54	56	50	29·90	0	Rain
7	47	48	46	·62	0	Rain
8	47	63	55	·63	42	Fair
9	56	65	56	·78	52	Fair
10	57	65	54	30·02	28	Cloudy
11	55	67	56	·02	21	Cloudy
12	57	68	52	·14	35	Fair
13	58	69	51	·20	51	Fair
14	55	67	55	·11	30	Cloudy
15	56	76	64	29·92	56	Fair
16	58	71	60	·72	61	Fair
17	56	67	56	·98	58	Fair
18	55	64	54	30·07	61	Fair
19	55	65	55	·21	62	Fair
20	57	67	54	·22	63	Fair
21	58	73	62	·36	64	Fair
22	59	71	61	·15	55	Fair
23	60	68	55	·13	54	Fair
24	57	74	60	·01	46	Fair
25	60	75	55	29·88	59	Fair
26	56	69	64	·96	41	Fair

N. B. The Barometer's height is taken at one o'clock.

XIV. *Theory of Galvanic Electricity, founded on Experience. By M.* I. A. Heidman, *Physician in Vienna. Abridged by M.* Guyton *.

In the first section of M. Heidman's work we find the history of Galvanic electricity, relatively to the discovery of the pile of Volta. The author includes under this head the observations of Galvani upon animal electricity; the experiments of Valli; the memoirs of Volta against the partisans of the system of a particular animal electricity; the dissertations of Fontana and of professor Reil; Du Creve's essay upon the irritability excited in a vacuum; the inquiries of Richard Fowler, of Pfaff, Humboldt, and Ritter; and, finally, he alludes to the experiments which led Volta to the discovery of the electrical pile, to which M. Heidman gives the name of *Galvanic battery.*

The second section contains the description of this apparatus, of the parts which compose it, and of their different dispositions. It is divided into five chapters, subdivided into articles, which treat successively of the simple Galvanic chain; of the nature of conductors; of their mass; of their extent; of the properties of solid or liquid conductors; and of the virtue attributed to solid and moistened conductors.

The author reviews the experiments and opinions of Aldini, Valli, Carminati, Volta, Vassali-Eandi, Davy, Humboldt, Fontana, Creve, Fowler, Pfaff, Pepys, Haldane, Ritter, Nicholson, Cavallo, Boekman, Arnim, Gilbert, Reinhold, &c.

It is in the three latter chapters of this section that M. Heidman presents his doctrine, and supports it by appropriate observations.

He relates two experiments which he made with the view of determining the influence of humid bodies in the Galvanic chain. The first consists in placing in contact two homogeneous metals (pure tin) with the nerves and muscles of a frog, and in establishing the communication of the two metals by means of zinc and silver: there is no contraction

* From *Annales de Chimie*, tom. lxi. p. 70.

either in closing or opening the chain, which is contrary to the opinion of Volta: there is here wanting, he says, the essential condition, being the humid, between the two heterogeneous metals.

In the second experiment he plunges the metallic plate into vessels where he has put salt water, and the electricity is augmented when we increase the quantity of the liquid in the vessels so as to make it touch a greater surface of the metals. He calls to mind the similar experiments of Messrs. Desormes and Van Marum, who have also concluded that the size of the humid surfaces contributes much to the strength of the pile.

M. Heidman afterwards endeavours to determine the power of the different fluid conductors, according to the degree of their chemical action.

For example, having placed two prepared frogs, in such a manner that their nerves were inserted in watch-glasses in which different liquids had been placed, such as a solution of alkaline sulphuret on one side, and water on the other side; the communication of the two fluids being established by a metallic wire, he observed that, at the moment of the formation of the chain, it was the muscle on the side of the first of these liquors which was contracted, and that from the moment of the rupture of the chain it was the muscle on the side of the water alone which suffered the contraction.

In order that he might not be led into error by the stimulant action peculiar to some liquors, and particularly to the acids, he took care to interpose, between the nerve and the liquor, a piece of flesh well soaked in water.

In this manner he submitted to experiment all the different liquids already known as conductors of ordinary electricity, and he has arranged them in the following order, relative to their chemical power and their Galvanic action:

Acids—oxymuriatic.
——— muriatic, impregnated with azote.
——— muriatic.
Solution of oxymuriate of potash.

Acids

Acids—sulphuric.
——— fluoric.
Sulphurets—alkaline.
————— earthy.
Acids—phosphoric.
——— arsenic.
——— oxalic.
——— boracic.
——— molybdic.
——— acetic.
——— benzoic.
——— gallic.
Solutions of ammonia.
————— potash.
————— soda.
————— phosphate of soda.
————— ammoniacal acetite.
————— ammoniacal muriate.
————— martial muriate.
————— ammoniacal nitrate.
————— ammoniacal sulphate.
————— muriate of potash.
————— nitrate of potash.
————— sulphate of potash.
————— phosphate of potash.
————— acetite of potash.
————— muriate of soda.
————— nitrate of soda.
————— sulphate of soda.
————— phosphate of soda.
————— acetite of soda.
————— muriate of barytes.
————— muriate of lime.
————— muriate of magnesia.
————— sulphate of alumine.
————— nitrate of silver.
————— sulphate of copper.
————— sulphate of iron.
————— sulphate of zinc.

Solutio

Solution of acetite of lead.
Citron juice.
Solution of acidulated tartrite of potash.
———— tartrite of potash.
———— tartrite of potash.
———— emetic tartar.
Water charged with carbonic acid gas.
Solution of ammoniacal carbonate.
———— carbonate of potash.
———— carbonate of soda.
Lime water.
Fresh urine.
Serum of the blood.
Blood.
White of eggs.
Fresh muscles.
Nerves, while still humid.
Water.
Sweetened water.
Saliva.
Juice of fresh plants.
Milk.
Wine.
Unrectified alcohol.
(Alcohol when rectified is not a conductor).

From his comparative experiments upon these liquids, he draws the following conclusion:

The oxidating liquid first in the order has the greatest chemical power, and determines the oxygen pole with the most of the solid conductors. The liquid of an inferior rank is therefore a simple conductor only, which in the apparatus for the decomposition of water indicates the hydrogen pole. The oxidating or irritating liquid, and the liquid conductor, must therefore be distinguished from each other. He observes, nevertheless, that the order he has assigned to these liquids is not constant, unless we employ them at an equal degree of concentration.

The author afterwards examines the action which these liquids exercise upon the various solid conductors which he regards

regards as depending upon chemical affinity, and which is manifested by the displacing of the poles in the same manner as with zinc: for instance, the acids go before the sulphurets and decide the oxygen pole (in the same way as with lead, tin, &c.); while with gold and platina the alkaline and earthy sulphurets assume the advantage even over the acids, and leave to these last the hydrogen pole only.

In treating of the Galvanic chain, M. Heidman has made two distinct classes.

He represents the former by this series: *metal more oxidizable—water—metal less oxidizable*; then the body communicating by immediate or intermediate contact with the two solid conductors. In this chain the oxygen pole is always according to the direction in which the two solid heterogeneous bodies touch the water.

He represents the Galvanic chain of the second class by this other series: *liquid which should oxidate—solid oxidizable conductor—liquid simply a conductor:* then follows the communication of the two heterogeneous liquids.

Thus in a chain formed of *silver—water—zinc—silver—water—zinc*, the first plate of silver and the last of zinc are (as he says) superfluous conductors; they cannot be considered as an essential part of the battery, since by these two plates we only complete the chain; and the two chains of six members are changed into a battery of two. Consequently, when we form the pile with plates of copper and zinc soldered, we should terminate it at the two extremities by simple plates, in order to suppress superfluous conductors, considering that it is not the placing of the copper or zinc at one of these extremities which decides the hydrogen and the oxygen pole, but only the respective disposition of the two heterogeneous metals, and their contact with water. Whence he concludes, that the plates by which we terminate the pile are superfluous members of the battery, contrary to the opinion of Volta, Carlisle, Nicholson, Reinhold, and all those who admit that the Galvanic action is produced only by the contact of two heterogeneous conductors, liquid or solid.

We find in the third section a detail of the phænomena

presented

presented spontaneously and without any foreign assistance, by the battery of Volta, when it is in full activity: the author reckons seven principal ones.

1. The peculiar smell it yields after some time, differing a little according to the nature of the conductors, which are generally metallic.

2. The slight noise or crackling produced by a strong battery, whether in the pile, or in the dish apparatus, and which proceeds from the hydrogen gas liberated sometimes even with a sort of white froth.

3. The changes undergone by the humid conductors in both of these kinds of apparatus, such as the decomposition of water, the saline efflorescences, the presence of a free alkali when neutral salts are employed, &c. &c.: the author regards the more minute examination of these changes as a vast field for new experiments.

4. The oxidation of the metals. He here embraces the opinion of those who regard it as essential to Galvanic action. It takes place, he says, when the chain is formed, not only in the atmospheric air, in the nitrous and oxygen gases, oxide of azote, in the acid gases, but also (although more feebly) in the vacuum of the air pump, and in the gases which contain no oxygen, such as hydrogen and azotic gases. He finds in these circumstances what principally constitutes the difference of the battery of Volta from the common electrical machine, in which there is no electricity produced without the presence of oxygen*. In the Galvanic battery oxygen is furnished by the water, by the liquors which contain it, and by the acids; its action is so much the more powerful, as this principle is the more abundant.

5. The absorption of oxygen. One of the most remarkable experiments is that in which professor Schaub observed the decomposition of common air under the bell glass; the cessation of action when the oxygen is exhausted: the renewing of the action, even with a commotion, after we have

* The author refers, for the development of some of these propositions, to the work he had previously published in two volumes under the title of Theory of Electricity.

supplied

supplied some fresh air; are phænomena which prove, according to M. Heidman, that this action requires oxygen, and should be ascribed to a chemical operation.

6. The immediate action of the battery of Volta upon Bennet's electrometer. When we bring this instrument near the hydrogen pole, the divergency of the gold leaves is stronger than when we bring it near the oxygen pole. Immediate contact is not necessary; it is sufficient when the base of the electrometer communicates with the earth, to establish a conductor in order to produce a divergency; certainly more feeble. It was not possible for the author to ascertain two opposite states of electricity of the hydrogen and oxygen poles. He employed with this view a very strong battery of Volta, and an electrometer made of a very narrow and very sensible glass cylinder: establishing, by means of a conductor, the communication of the hydrogen pole with the top of the instrument, the divergency nearly carried the gold leaves to the sides; and they remained in the same situation when he established the communication with the oxygen pole, which would be impossible in the case of two contrary electricities. If we put in communication, by means of a good metallic conductor, the battery of Volta with a prepared frog, which does not communicate with the ground, there is no movement; a proof that in every Galvanic action there must be in reality a discharge or a partition of the Galvanic electricity, which has not taken place in this case.

7. The seventh article, which finishes this section, has for its object the spark of the battery of Volta, as seen in the dark by Nicholson. The author also relates the observation of Pfaff, Hebebrandt, Biot, and Hallé, that, independently of the closing of the chain by the two extremities of the battery, there often appears a light upon the pile itself, that is to say, upon the sides of the metallic plates. M. Heidman thinks himself well founded in doubting this phænomenon, the observations of which appear to him not to have been precise, or reconcileable with received principles. He presumes that in the dark we may close by the conductor a

part only of the battery, from which the spark has been drawn.

We may judge from this hasty sketch that M. Heidman is perfectly master of his subject; that the numerous facts which at present form the whole of our knowledge on Galvanic electricity are classed in his work with method and brevity. The publication of his first volume makes us wish to see his continuation so much the more anxiously, as M. Heidman does not always agree with those whose opinions he details, and because he supports his own observations upon experiments peculiar to himself, and of which he solicits the verification.

XV. *The Bakerian Lecture, on some Chemical Agencies of Electricity.* By HUMPHRY DAVY, *Esq. F. R. S. M. R. I. A.*

[Continued from p. 18.]

IV. *On the Transfer of certain of the constituent Parts of Bodies by the Action of Electricity.*

M. GAUTHEROT has stated*, that in a single Galvanic circle of zinc, silver, and water, in an active state, the oxide of zinc formed is attracted by the silver †; and Messrs. Hisinger and Berzelius detail an account of an experiment in which solution of muriate of lime being placed in the positive part of a syphon, electrified by wires from a Voltaic pile, and distilled water in the negative part, lime appeared in the distilled water.

These facts rendered it probable that the saline elements evolved in decompositions by electricity were capable of being transferred from one electrified surface to another, according to their usual order of arrangement; but to demonstrate this clearly, new researches were wanting.

I connected one of the cups of sulphate of lime, mentioned page 12, with a cup of agate by asbestus; and, filling them

* *Annales de Chimie*, vol. xxxxix. p. 203. † Ibid. vol. li. p. 171.

them with purified water, made the platina wire in the cup of sulphate of lime transmit the electricity from a power of 100: a wire in the agate cup received it. In about four hours a strong solution of lime was found in the agate cup, and sulphuric acid in the cup of sulphate of lime. By reversing the order, and carrying on the process for a similar time, the sulphuric acid appeared in the agate cup, and the solution of lime on the opposite side.

Many trials were made with other saline substances, with analogous results. When the compounds of the strong mineral acids with alkaline or alkaline-earthy bases were introduced into one tube of glass, distilled water connected by amianthus being in another tube, both connected by wires of platina in the Voltaic arrangement, the base always passed into the distilled water when it was negative, and the acid when it was positive.

The metals and the metallic oxides passed towards the negative surface like the alkalies, and collected round it. In a case in which solution of nitrate of silver was used on the positive side, and distilled water on the negative, silver appeared on the whole of the trasmitting amianthus, so as to cover it with a thin metallic film.

The time required for these transmissions (the quantity and intensity of the electricity, and other circumstances, remaining the same) seemed to be in some proportion as the length of the intermediate volume of water. Thus, when with the power of 100, sulphate of potash was on the negative side, and distilled water on the positive side, the distance between the wires being only an inch, sulphuric acid, in sufficient quantity to be very manifest, was found in the water in less than five minutes; but when the tubes were connected by an intermediate vessel of pure water, so as to make the circuit 8 inches, 14 hours were required to produce the same effect.

To ascertain whether the contact of the saline solution with a metallic surface was necessary for the decomposition and transfer, I introduced purified water into two glass tubes; a vessel containing solution of muriate of potash was connected with them respectively by amianthus; and the arrangement

arrangement was made in such a way, that the level of both the portions of purified water was higher than the level of the saline solution.

In this case the saline matter was distant from each of the wires at least two-thirds of an inch; yet alkaline matter soon appeared in one tube, and acid matter in the other: and in 16 hours moderately strong solutions of potash and of muriatic acid had been formed.

In this case of electrical transfer or attraction the acid and alkaline matter seemed to be perfectly pure, and I am inclined to believe that this is uniformly the case in all experiments carefully made. One of the instances in which I conceived acid most likely to be present, was in the transfer of magnesia from sulphate of magnesia in the positive tube to distilled water in the negative tube. I examined the case, taking care that the distilled water was never upon a lower level than the saline solution: the process was continued for some hours, till a considerable quantity of magnesia had appeared. The connecting amianthus was removed, and muriatic acid poured into the tube: the saturated solution did not precipitate solution of muriate of barytes.

I endeavoured to ascertain the progress of the transfer, and the course of the acid or alkaline matter in these decompositions, by using solutions of litmus and turmeric, and papers coloured by these substances; and these trials led to the knowledge of some singular and unexpected circumstances.

Two tubes, one containing distilled water, the other solution of sulphate of potash, were each connected by amianthus with a small ounce measure filled with distilled water tinged by litmus: the saline solution was negatively electrified; and as it was natural to suppose that the sulphuric acid in passing through the water to the positive side would redden the litmus in its course, some slips of moistened paper tinged with litmus were placed above and below the pieces of amianthus, directly in the circuit. The progress of the experiment was minutely observed: the first effect of reddening took place immediately above the positive surface, where I had least expected it; the red tint slowly diffused itself from the positive side to the middle of the vessel,

but

but no redness appeared above the amianthus, or about it, on the negative side; and though it had been constantly transmitting sulphuric acid, it remained unaffected to the last.

The order of the experiment was changed, and the saline solution placed on the positive side; a solution and papers tinged with turmeric being substituted for those tinged with litmus. The effect was precisely analogous; the turmeric became brown first near the negative wire, and no change took place in the intermediate vessel near the positive wire.

In another process the two glass tubes were filled with solution of muriate of soda, and the intermediate vessel with solution of sulphate of silver; paper tinged with turmeric was placed on the positive side, and paper tinged with litmus on the negative side: as soon as the electrical circuit was complete, soda began to appear in the negative tube, and oxymuriatic acid in the positive tube; and the alternate products were exhibited passing into the solution of sulphate of silver, the muriatic acid occasioning a dense heavy precipitate, and the soda a more diffused and a lighter one; but neither the turmeric transmitting the alkali, nor the litmus transmitting the acid, had their tints in the slightest degree altered.

V. *On the Passage of Acids, Alkalies, and other Substances, through various attracting chemical Menstrua, by means of Electricity.*

As acid and alkaline substances, during the time of their electrical transfer, passed through water containing vegetable colours without affecting them, or apparently combining with them, it immediately became an object of inquiry, whether they would not likewise pass through chemical menstrua having stronger attractions for them; and it seemed reasonable to suppose that the same power which destroyed elective affinity in the vicinity of the metallic points, would likewise destroy it, or suspend its operation, throughout the whole of the circuit.

An arrangement was made of the same vessels and apparatus employed in the experiment on the solution of muriate

of

of soda and sulphate of silver, page 107. Solution of sulphate of potash was placed in contact with the negatively electrified point, pure water was placed in contact with the positively electrified point, and a weak solution of ammonia was made the middle link of the conducting chain; so that no sulphuric acid could pass to the positive point in the distilled water without passing through the solution of ammonia.

The power of 150 was used: in less than five minutes it was found, by means of litmus paper, that acid was collecting round the positive point; in half an hour the result was sufficiently distinct for accurate examination.

The water was sour to the taste, and precipitated solution of nitrate of barytes.

Similar experiments were made with solution of lime, and weak solutions of potash and soda, and the results were analogous. With strong solutions of potash and soda a much longer time was required for the exhibition of the acid; but even with the most saturated alkaline lixivium it always appeared in a certain period.

Muriatic acid from muriate of soda, and nitric acid from nitrate of potash, were transmitted through concentrated alkaline menstrua under similar circumstances.

When distilled water was placed in the negative part of the circuit, and a solution of sulphuric, muriatic, or nitric acid in the middle, and any neutral salt with a base of lime, soda, potash, ammonia, or magnesia, in the positive part, the alkaline matter was transmitted through the acid matter to the negative surface, with similar circumstances to those occurring during the passage of the acid through the alkaline menstrua; and the less concentrated the solution, the greater seemed to be the facility of transmission.

I tried in this way muriate of lime with sulphuric acid, nitrate of potash with muriatic acid, sulphate of soda with muriatic acid, and muriate of magnesia with sulphuric acid; I employed the power of 150; and in less than 48 hours I gained in all these cases decided results; and magnesia came over like the rest.

Strontites and barytes passed, like the other alkaline sub-

 stances,

stances, readily through muriatic and nitric acids; and, *vice versâ*, these acids passed with facility through aqueous solutions of barytes and strontites: but in experiments in which it was attempted to pass sulphuric acid through the same menstrua, or to pass barytes or strontites through this acid, the results were very different.

When solution of sulphate of potash was in the negative part of the circuit, distilled water in the positive part, and saturated solution of barytes in the middle, no sensible quantity of sulphuric acid existed in the distilled water after thirty hours, the power of 150 being used: after four days, sulphuric acid appeared, but the quantity was extremely minute: much sulphate of barytes had formed in the intermediate vessel; the solution of barytes was so weak as barely to tinge litmus; and a thick film of carbonate of barytes had formed on the surface of the fluid. With solution of strontites the result was very analogous, but the sulphuric acid was sensible in three days.

When solution of muriate of barytes was made positive by the power of 150, concentrated sulphuric acid intermediate, and distilled water negative, no barytes appeared in the distilled water when the experiment had been carried on for four days; but much oxymuriatic acid had formed in the positive vessel, and much sulphate of barytes had been deposited in the sulphuric acid.

Such of the metallic oxides as were made subjects of experiment passed through acid solutions from the positive to the negative side, but the effect was much longer in taking place than in the instances of the transition of alkaline matter. When solution of green sulphate of iron was made positive, solution of muriatic acid intermediate, and water negative, in the usual arrangement, green oxide of iron began to appear in about ten hours upon the negative connecting amianthus, and in three days a considerable portion had been deposited in the tube. Analogous results were obtained with sulphate of copper, nitrate of lead, and nitro-muriate of tin.

I made several experiments on the transition of alkaline and

and acid matter through different neutrosaline solutions, and the results were such as might well have been anticipated.

When solution of muriate of barytes was negative, solution of sulphate of potash intermediate, and pure water positive, the power being from 150, sulphuric acid appeared in about five minutes in the distilled water; and in two hours the muriatic acid was likewise very evident. When solution of sulphate of potash was positive, solution of muriate of barytes intermediate, and distilled water negative, the barytes appeared in the water in a few minutes; the potash from the more remote part of the chain was nearly an hour in accumulating, so as to be sensible.

When the solution of muriate of barytes was positive, the solution of sulphate of potash intermediate, and distilled water negative, the potash soon appeared in the distilled water; a copious precipitation of sulphate of barytes formed in the middle vessel; but after ten hours no barytes had passed into the water.

When solution of sulphate of silver was interposed between solution of muriate of barytes on the negative side and pure water on the positive side, sulphuric acid alone passed into the distilled water; and there was a copious precipitation in the solution of sulphate of silver. This process was carried on for ten hours.

I tried several of these experiments of transition upon vegetable and animal substances with perfect success.

The saline matter exposed in contact with the metal, and that existing in the vegetable or animal substances, both underwent decomposition and transfer; and the time of the appearance of the different products at the extremities of the circuit was governed by the degree of their vicinity.

Thus when a fresh leaf-stalk of the polyanthus, about two inches long, was made to connect a positively electrified tube containing solution of nitrate of strontites, and a negatively electrified tube containing pure water, the water soon became green, and gave indications of alkaline properties, and free nitric acid was rapidly separated in the positive tube. After ten minutes the alkaline matter was examined;

mined; it consisted of potash and lime, and as yet no strontites had been carried into it: for the precipitate it gave with sulphuric acid readily dissolved in muriatic acid. In half an hour strontites, however, appeared; and in four hours it formed a very abundant ingredient of the solution.

A piece of muscular flesh of beef, of about three inches in length and half an inch in thickness, was treated in the same way as the medium of communication between muriate of barytes and distilled water. The first products were soda, ammonia, and lime; and after an hour and a quarter, the barytes was very evident. There was much free oxymuriatic acid in the positively electrified tube, but no particle of muriatic acid had passed into the negative tube, either from the muriatic solution or from the muscular fibre.

VI. *Some general Observations on these Phænomena, and on the Mode of Decomposition and Transition.*

It will be a general expression of the facts that have been detailed, relating to the changes and transitions by electricity, in common philosophical language, to say that hydrogen, the alkaline substances, the metals, and certain metallic oxides, are attracted by negatively electrified metallic surfaces, and repelled by positively electrified metallic surfaces; and contrariwise, that oxygen and acid substances are attracted by positively electrified metallic surfaces, and repelled by negatively electrified metallic surfaces; and these attractive and repulsive forces are sufficiently energetic to destroy or suspend the usual operation of elective affinity.

It is very natural to suppose that the repellent and attractive energies are communicated from one particle to another particle of the same kind, so as to establish a conducting chain in the fluid; and that the locomotion takes place in consequence; and that this is really the case seems to be shown by many facts. Thus, in [illegible] the instances in which I examined alkaline solutions through which acids had been transmitted, I always found acid in them whenever any acid matter remained at the original source. In time, by the attractive power of the positive surface, the decomposition and transfer undoubtedly become complete; but this does not affect the conclusion.

In

In the cases of the separation of the constituents of water, and of solutions of neutral salts forming the whole of the chain, there may possibly be a succession of decompositions and recompositions throughout the fluid. And this idea is strengthened by the experiments on the attempt to pass barytes through sulphuric acid, and muriatic acid through solution of sulphàte of silver, in which, as insoluble compounds are formed and carried out of the sphere of the electrical action, the power of transfer is destroyed. A similar conclusion might likewise be drawn from many other instances. Magnesia and the metallic oxides, as I have already mentioned, will pass along moist amianthus from the positive to the negative surface; but if a vessel of pure water be interposed, they do not reach the negative vessel, but sink to the bottom. These experiments I have very often made, and the results are perfectly conclusive; and in the case, page 109, in which sulphuric acid seemed to pass in small quantities through very weak solutions of strontites and barytes, I have no doubt but that it was carried through by means of a thin stratum of pure water, where the solution had been decomposed at the surface by carbonic acid; for in an experiment similar to these in which the film of carbonate of barytes was often removed and the fluid agitated, no particle of sulphuric acid appeared in the positive part of the chain.

It is easy to explain, from the general phænomena of decomposition and transfer, the mode in which oxygen and hydrogen are separately evolved from water. The oxygen of a portion of water is attracted by the positive surface, at the same time that the other constituent part, the hydrogen, is repelled by it; and the opposite process takes place at the negative surface; and in the middle or neutral point of the circuit, whether there be a series of decompositions and recompositions, or whether the particles from the extreme points only are active, there must be a new combination of the repelled matter: and the case is analogous to that of two portions of muriate of soda separated by distilled water; muriatic acid is repelled from the negative side, and soda from the positive side, and muriate of soda is composed in the middle vessel.

These

These facts seem fully to invalidate the conjectures of M. Ritter, and some other philosophers, with regard to the elementary nature of water, and perfectly to confirm the great discovery of Mr. Cavendish.

M. Ritter conceived that he had procured oxygen from water without hydrogen, by making sulphuric acid the medium of communication at the negative surface; but in this case sulphur is deposited, and the oxygen from the acid and the hydrogen from the water are respectively repelled, and a new combination produced.

I have attempted some of the experiments of decomposition and transfer by means of common electricity, making use of a powerful electrical machine of Mr. Nairne's construction, belonging to the Royal Institution, of which the cylinder is 15 inches in diameter, and two feet long.

With the same apparatus as that employed for decompositions by the Voltaic battery, no perceptible effect was produced by passing a strong current of electricity silently for four hours through solution of sulphate of potash.

But by employing fine platina points of 1 70th of an inch in diameter, cemented in glass tubes in the manner contrived by Dr. Wollaston *, and bringing them near each other, in vessels containing from three to four grains of the solution, and connected by moist asbestus, potash appeared in less than two hours round the negatively electrified point, and sulphuric acid round the positive point.

In a similar experiment sulphuric acid was transferred through moist asbestus into water; so that there can be no doubt that the principle of action is the same in common and the Voltaic electricity †.

VII. On

* Phil. Trans. vol. xci. p. 427.

† This had been shown, with regard to the decomposition of water, by Dr. Wollaston's important researches. By carefully avoiding sparks, I have been able to obtain the two constituents in a separate state. In an experiment in which a fine platina point cemented in glass, and connected by a single wire with the positive conductor of this machine, was plunged in distilled water in an insulated state, and the electricity dissipated into the atmosphere by means of moistened filaments of cotton, oxygen gas mixed with a little nitrogen gas was produced; and when the same apparatus was applied to the negative conductor hydrogen gas was evolved, and a minute portion of oxygen and nitrogen

VII. *On the general Principles of the chemical Changes produced by Electricity.*

The experiments of Mr. Bennet had shown that many bodies brought into contact and afterwards separated, exhibited opposite states of electricity; but it is to the investigations of Volta that a clear development of the fact is owing; he has distinctly shown it in the case of copper and zinc, and other metallic combinations; and has supposed that it also takes place with regard to metals and fluids.

In a series of experiments made in 1801 * on the construction of electrical combinations by means of alternations of single metallic plates, and different strata of fluids, I observed that when acid and alkaline solutions were employed as elements of these instruments, the alkaline solutions always received the electricity from the metal, and the acid always transmitted it to the metal: thus, in an arrangement of which the elements were tin, water, and solution of potash, the circulation of the electricity was from the water to the tin, and from the tin to the solution of potash; but in an arrangement composed of weak nitric acid, water, and tin, the order was from the acid to the tin, and from the tin to the water.

These principles seem to bear an immediate relation to the general phænomena of decomposition and transference, which have been the subject of the preceding details.

In the simplest case of electrical action, the alkali which receives electricity from the metal would necessarily, on being separated from it, appear positive, whilst the acid under similar circumstances would be negative; and these bodies having respectively, with regard to the metals, that which may be called a positive and a negative electrical

nitrogen gases: but neither of the foreign products, the nitrogen gas in the one case, and the nitrogen and oxygen gases in the other, formed as much as 1-50th part of the volume of the gases; and there is every reason to suppose that they were derived from the extrication of common air, which had been dissolved in the water. This result, which when I first obtained it in 1803 appeared very obscure, is now easily explained; the alternate products must have been evolved at the points of the dissipation of the electricity.

* See Phil. Trans. vol. xci. p. 397.

energy,

energy, in their repellent and attractive functions seem to be governed by laws the same as the common laws of electrical attraction and repulsion; the body possessing the positive energy being repelled by positively electrified surfaces, and attracted by negatively electrical surfaces; and the body possessing the negative energy following the contrary order.

I have made a number of experiments with the view of elucidating this idea, and of extending its application; and in all cases they have tended to confirm the analogy in a remarkable manner.

Well burned charcoal water and nitric acid, the same substance water and solution of soda, made respectively elements of different electrical combinations, became distinctly active when twenty alternations were put together; the positive energy being exhibited on the side of the alkali, and the negative on that of the acid. Arrangements of plates of zinc, pieces of moistened pasteboard, and moistened quicklime, to the number of forty series, likewise formed a weak electrical pile, the effect of the lime being similar to that of an alkali, but the power was soon lost.

I endeavoured, by means of very delicate instruments, to ascertain the electrical states of single insulated acid and alkaline solutions, after their contact with metals; and for this purpose I employed at different times the condensing electrometer of Mr. Cuthbertson's construction, Mr. Cavallo's multiplier, and a very sensible electrical balance, on the principle of torsion, adopted by M. Coulomb: but the effects were unsatisfactory; the circumstances of evaporation and of chemical action, and the adherence of the solutions to the surfaces of the metals employed, in most cases prevented any distinct result, or rendered the source of the electricity doubtful. I shall not enter into any details of these processes, or attempt to draw conclusions from capricious and uncertain appearances, which, as we shall immediately see, may be fully deduced from clear and distinct ones.

The alkaline and acid substances capable of existing in the dry and solid form, give by contact with the metals exceedingly

ceedingly sensible electricities, which require for their exhibition the gold leaf electrometer only with the small condensing plate.

When oxalic, succinic, benzoic or boracic acid, perfectly dry, either in powder or crystals, were touched upon an extended surface with a plate of copper insulated by a glass handle, the copper was found positive, the acid negative. In favourable weather, and when the electrometer was in perfect condition, one contact of the metal was sufficient to produce a sensible charge; but seldom more than five or six were required. Other metals, zinc and tin for instance, were tried with the same effect. And the metal received the positive charge, apparently to the same extent, whether the acid was insulated upon glass, or connected with the ground.

The solid acid of phosphorus, which had been strongly ignited, and most carefully excluded from the contact of air, rendered the insulated plate of zinc positive by four contacts; but after exposure to the atmosphere for a few minutes it wholly lost this power.

When metallic plates were made to touch dry lime, strontites, or magnesia, the metal became negative; the effect was exceedingly distinct, a single contact upon a large surface being sufficient to communicate a considerable charge. For these experiments the earths were carefully prepared; they were in powder, and had been kept for several days in glass bottles before they were used: it is essential to the success of the process that they be of the temperature of the atmosphere. In some experiments which I made upon them when cooling, after having been ignited, they appeared strongly electrical, and rendered the conductors brought in contact with them positive.

I made several experiments, in a similar manner, on the effects of the contact of potash and soda with the metals. Potash in no instance afforded a satisfactory result; its powerful attraction for water presents an obstacle probably unsurmountable to the success of any trials made in the free atmosphere. Soda, in the only case in which electricity was exhibited, affected the metal in the same way as lime,

strontites,

strontites, and magnesia. Upon this occasion the soda had been prepared with great care, exposed in a platina crucible for nearly an hour in a red heat, and suffered to cool in the crucible inverted over mercury; when cool it was immediately removed, and the contact made with a plate of zinc: the experiment was performed in the open air; the weather was peculiarly dry, the thermometer stood at 26° Fahrenheit, and the barometer at 30·2 inches; six contacts gave a charge to the condensing electrometer in the first trial; in the second ten were required to produce a similar effect; and after this, though two minutes only had elapsed, no further result could be obtained.

In the decomposition of sulphuric acid by Voltaic electricity the sulphur separates on the negative side. The experiments of various electricians prove, that by the friction of sulphur and metals the sulphur becomes positive and the metals negative; the same thing I find happens from the contact of an unexcited cake of sulphur and insulated metallic plates. Mr. Wilke has stated an exception to lead, as rendering sulphur negative by its friction. The results that I have obtained with lead, in trials very carefully made, are the same as those with other metals*. Sulphur, by being rubbed or struck against newly-polishsd lead, always became positive. Mr. Wilke, perhaps, was misled by using tarnished lead: sulphur, I find, rubbed against litharge, or lead the surface of which has been long exposed to air, becomes negative; and this exception being removed, all the facts on the subject are confirmations of the general principle †.

On

* As sulphur is a nonconductor, and easily excited by slight friction, or small changes in its temperature, some caution is required in drawing conclusions from the experiments in which it is employed. Sulphur, examined immediately after having been heated, gives a positive charge to conductors, agreeing in this respect with the alkaline substances; and a slight contact with the dry hand is sufficient to render it negative. In general, likewise, in experiments of contact care should be taken that the metallic plate is free from electricity: well polished plates of copper and zinc will, I find, receive a negative charge from being laid on a table of common mahogany.

† Concentrated solution of phosphoric acid, I find, is decomposed by Voltaic electricity: the phosphorus combines with the negatively electrified

metal

On the general principle oxygen and hydrogen ought to possess, with regard to the metals respectively, the negative and positive energy. This I have not been able to prove by direct experiments of contact; but the idea is confirmed by the agency of their compounds: thus I have found that solution of sulphuretted hydrogen in water acts in the electrical apparatus, composed of single plates and different strata of fluids, in the same manner as alkaline solutions; and that solution of oxymuriatic acid is more powerful in similar arrangements than solutions of muriatic acid of a higher degree of concentration; and in both these cases it is impossible to conceive the combined hydrogen and oxygen inactive. The inference, likewise, is fully warranted by the case of the solutions of alkaline hydroguretted sulphurets, which consisting principally of alkali and sulphur together in union with water, exhibit the positive energy with regard to the metals in a very high degree. In the series of experiments on Voltaic arrangements, constructed with single plates above mentioned, I found the solutions of hydroguretted sulphurets in general much more active than alkaline solutions, and particularly active with copper, silver, and lead. And in an experiment that I made on a combination of copper, iron, and hydroguretted sulphurets of potash, in 1802, I found that the positive energy of the hydroguretted sulphurets with regard to the copper was sufficient to overpower that of the iron; so that the electricity did not circulate from the copper to the iron, and from the iron to the fluid, as in common cases, but from the copper to the hydroguretted sulphuret, and from the hydroguretted sulphuret to the iron.

All these details afford the strongest confirmation of the principle. It may be considered almost as a mere arrange-

metal, and forms a phosphuret; at least this happened in the two cases that I tried with platina and copper. From all analogy it may be inferred that the electrical energy of this inflammable substance with regard to metals is the same as that of sulphur. I tried some experiments of contact upon it, but without success. Its slow combustion in the atmosphere, it is most likely, was the cause of the failure; but even in gases not containing free or loosely combined oxygen, its evaporation would probably interfere.

ment

ment of facts; and, with some extensions, it seems capable of being generally applied.

Bodies possessing opposite electrical energies with regard to one and the same body, we might fairly conclude, would likewise possess them with regard to each other. This I have found, by experiment, is the case with lime and oxalic acid. A dry piece of lime, made from a very pure compact secondary limestone, and of such a form as to present a large smooth surface, became positively electrical by repeated contacts with crystals of oxalic acid; and these crystals placed upon the top of a condensing electrometer, and repeatedly touched by the lime, which after each contact was freed from its charge, rendered the gold leaves negatively electrical. The tendency of the mere contacts of the acid and alkali with the metal would be to produce opposite effects to those exhibited, so that their mutual agency must have been very energetic.

It will not certainly be a remote analogy to consider the other acid and alkaline substances generally, and oxygen and hydrogen as possessing similar electrical relations; and in the decompositions and changes presented by the effects of electricity, the different bodies naturally possessed of chemical affinities appear incapable of combining, or of remaining in combination, when placed in a state of electricity different from their natural order. Thus, as we have seen, the acids in the positive part of the circuit separate themselves from alkalies, oxygen from hydrogen, and so on; and metals on the negative side do not unite to oxygen, and acids do not remain in union with their oxides; and in this way the attractive and repellent agencies seem to be communicated from the metallic surfaces throughout the whole of the menstruum.

[To be continued.]

XVI. *On*

XVI. *On the Dislocations of the Strata of the Earth.* *By Mr.* John Farey.

To Mr. Tilloch.

Sir,

Two passages in my letter to you of the 21st of May 1806 (vol. xxv. p. 45 and 46), having occasioned inquiries from different friends, as to what I therein meant, by the operations of *gravity* upon the *strata*, "in circumstances which have never yet been contemplated among the physical inquiries of mathematicians," I beg the indulgence of your readers (if you shall be pleased to favour me by the insertion of this) to state, that some few months after I had become acquainted with Mr. *William Smith's* discoveries respecting the strata, principally from considering the worn state of the edges of the strata in the fissures, "I was led to consider an extension of the principles of gravity, which now operate in producing the tides of the ocean, to the case of a large satellitic body once probably revolving near to the surface of the earth, and whose attraction *heaved up* the land, at the short intervals of return in its orbit, for restoring the equilibrium, quicker than the waters could run to form a tide capable of so doing, as at present. By these supposed tides, and the ultimate fall of the satellite occasioning them, into the mass of the earth, it should seem that I shall be able to account for all the changes, observable or probable, between the state of a sphere whose surface was uniformly covered with water, nearly quiescent, and under which its stratification had proceeded with the most perfect regularity (something similar to crystallization) to the ruptured, abraded, and dislocated form, in which the dry land now appears, projecting above the nearly spheroidal figure assumed by the ocean."

The articles *Coal*, *Collieries*, and others expected to succeed them in *Dr. Rees's New Cyclopædia*, will, it is hoped, tend to a further elucidation of this subject. I am, sir,

Your obedient servant,

John Farey.

12, Upper Crown-street, Westminster,
July 14, 1807.

XVII. *His-*

XVII. *History of Astronomy for the Year* 1806. *By* Jerome de Lalande.

[Continued from p. 79.]

In the Ephemerides of Milan we find observations upon the three new planets, the opposition of Jupiter in 1804, the eulogium upon Reggio, a grand work of M. Oriani for the calculation of spheroids, some doubts of M. Cesaris upon the parallax of the stars announced by Messrs. Piazzi and Calandrelli, a memoir of M. Carlini upon the refractions, where he gives some analytical formulæ, and some observations upon the stars round the poles. In order to deduce from them the coefficients, he announces the tables of refractions, which will be supported upon it.

M. Oriani, one of the astronomers of Milan, has refused to be made a bishop, and the emperor has given him a pension of 6000 livres of Milan.

The Ephemerides of M. Bode, for 1806, which have not yet reached us, contain observations, memoirs of M. Bode, of Messrs. Mechain at Paris, Triesnecker at Vienna, David at Prague, Beittler at Mittau, Wurm at Blanburen, Bugge at Copenhagen, Derflinger at Kremsmunster, Schroeter at Lilienthal, Jungnitz at Breslau, Benzenberg at Hamburg, Harding at Lilienthal, Olbers at Bremen, Gauss at Brunswick, Fritsch at Quedlingbourg, Seyfert at Dresden, Ende at Celle, Prosperin at Upsal, Kautsch at Leutomischel, Schubert at Petersburg, Brandes at Eckwarden, Hahn at Remplin, Melanderhielm at Stockholm, Klugel at Halle, Koch at Dantzic, Maskelyne's new Catalogue, Letters from Messrs. Piazzi, Lalande, Ideler, Fuss, Eimbecke, Repsold, Wechmar, Reissig, Soldner, and Burja. We may judge by this catalogue of the astronomical activity of the Germans; I wish there was as much in France.

M. Bode has given a new edition enlarged of his small celestial atlas of 1782 in 34 sheets, with a catalogue of 5000 stars.

M. Burja has finished his astronomy, in 5 volumes in 8vo.

I published in the Magasin Encyclopédique for the month

month of August a singular calculation made by an anonymous astronomer in Germany, who ascertained that the six old planets would return to the same spot in 280,000 years. M. Mougin has ascertained that in fact the differences do not exceed the errors that the longitude of this period should produce in the revolutions of 2000 years, or rather for 200 years; because the observations which are more antient are very imperfect.

M. Poczobut, notwithstanding his great age, still furnishes an example of zeal and courage. He has sent me a course of curious observations made upon Harding's planet, along with M. Reschka, professor of astronomy at Wilna in Poland.

M. Gauss has calculated the six elements of this planet, which differ very little from the preceding ones, but which will require alterations when this skilful geometrician shall have finished the calculation of the perturbations with which he is occupied.

M. Schroter has made some new inquiries upon the position of the axis of Mars, upon its atmosphere, and its spots.

The Memoirs of Berlin for 1803, which appeared in the month of May 1805, contain a great number of observations of M. Bode made at the royal observatory of Berlin.

The eclipse of the sun on the 6th of June was the most remarkable phænomenon of the year 1806, because it was total in the United States. It was observed in three places in America, and served me for calculating the solar and lunar diameters by comparing it with the observations of the annular eclipses, as may be seen in the Memoirs of the Institute. I am surprised it was not observed at Boston, where there is an academy; but Mr. Deferrer, an Englishman settled at New York, went to Albany on purpose to see it.

M. Richard, a missionary on the banks of Lake Erie, also observed it; and I conclude the difference of meridians to be $5^{h}\ 36'$; which confirms the position given in the chart of the United States by Abraham Bradley.

This eclipse was observed at fifteen places in Europe; whence I determined the time of the conjunction at $4^{h}\ 30'\ 6''$ true

true time at Paris, and the latitude 19′ 20″. The correction of the tables of M. Burg is 28ˢ, and in latitude 3″.

M. Goudin has calculated several of these observations, by an analytical method derived from that which he published in order to calculate, by anticipation, the phases of an eclipse for all the countries on the globe.

They have inserted in the *Connoissance des Tems* the chart of the eclipse of the sun on the 29th of November 1807; but they have omitted the general disposition calculated for all latitudes by M. Goudin, and that of the principal cities by M. Duvaucel; they will be placed in the volume for 1809, which will be published previous to that eclipse.

M. Delaplace has given in the *Journal de Physique* some memoirs in which he shows that the adhesion of bodies, placed upon the surface of a fluid, corresponds with the capillary attraction of which he has given the mathematical theory, and he shows the method of calculating this adhesion according to the experiments of Messrs. Haüy and Achard. The principle of attraction between the molecules of bodies decreasing with an extreme rapidity, which expresses the capillary phænomena, is also the cause of the chemical affinities; it produces an influence of masses, the effects of which M. Berthollet has developed in a new and happy manner.

M. Arrago, seeing that the light reflected by the satellites gives the same velocity with the aberration of the stars, concludes from this that the velocity does not change. M. Arrago found the same thing with terrestrial objects: he made experiments with a prism applied to the mural quadrant, upon the light of the sun, the stars, and terrestrial objects; and he found that the velocity of the light is the same in every circumstance.

M. Halma, bookseller to the empress, has undertaken a translation into French of the Almagestus of Ptolemy.

M. Humboldt is busy at Berlin in editing his Historical, Physical, and Political Travels; he is also occupied with the horary variations of magnetism. M. Oltmanns labours along with him; this young geometrician calculates with as much zeal as intelligence; he is about to publish a volume of astronomical

astronomical observations, and he now meditates a journey into Asia.

The city of Erlang, seven leagues north from Nuremberg, and which belonged to the kingdom of Prussia, has been treated with the greatest possible mildness: for this protection it has been indebted to its distinguished university, which enjoys the special protection of the French generals.

The medal which the Institute decrees every year towards the equinox, for the best work upon astronomy according to my foundation, has been adjudged to M. Svanberg, a Swedish astronomer, who has published the measure of the degree in Lapland, by means of which we have ascertained the error the causes of which are pointed out in the History of Astronomy for 1805. The medal represents the observatory, and upon the reverse—*Præmium astronomicum Instituti Gallici.*

John Svanberg was born upon the 7th of April 1771 in the parish of Calixe, thirteen leagues from Tornea. He had an uncle who took charge of his education, and who designed him for the church; but the first opportunity he had of seeing a book of mathematics, being a Life of Maclaurin, decided his taste. As soon as he left the university, he gave himself up to the study of astronomy, with the assistance of M. Nordmark, a geometrician of great merit.

In 1796 he was made vice-secretary to the Stockholm Academy, and in 1803 director of the observatory.

The Academy of Copenhagen proposes as a prize question, to ascertain if there be a maximum and minimum in the changes, produced by the perturbations upon the orbits of the planets, which depends upon the nature of the orbits. The prize is 400 francs; the pieces will be received until the end of 1807.

The Academy of Berlin has extended the time for determining the prize upon the variations of obliquity for two years longer.

M. Delambre has finished the editing of all the observations of stars and latitudes for the meridian. There is still wanting the calculation of the arcs and the latitudes for the second

second volume, and afterwards the comparison of the old meridian. The basis of Rodez is bad: 22 toises have been added at the degree between Perpignan and Rodez, which departs from the progression. It is not the attraction of mountains, but the base which is the cause of it; even the angles upon that base were bad.

M. Bouvard has finished the printing of his new tables of Jupiter and Saturn. I have completed those of Mercury and Venus, which are ready for printing. M. Delambre has finished those of the first satellite with the new perturbations.

M. Firmin Didot has furnished, as a specimen of French industry, a table of sines calculated to seconds, which are carried one place further than those of Taylor. M. de Prony will publish the prospectus, and M. Didot has added to it a page in folio for the natural sines to 22 places for the ten thousandths.

M. Barry, at Manheim, announces a collection of observations. He placed a distant mark for his meridian glass.

At the end of July a report was circulated that M. de Zach had quitted the observatory of Gotha, which he had rendered so celebrated; but as it was he who obtained its construction, astronomy will always have the advantage of his zeal, his influence, and his labours. His journal has been continued: we have seen in it, among other things, the position of Eisenberg, at present remarkable as being the residence of the learned duchess dowager of Gotha, 50° 58′ 3″ latitude, and 34′ 29″ east from Paris. We there see also the diminution of the Baltic Sea of 45 inches in the century, and the supposition of the junction of Asia with America, from the voyages of the Russians, &c.

At Paris, on the 31st of October, the pyramid of Mont Rouge was finished; it is intended to serve as a mark for the meridian glass: the trees around it are cut down.

The Bureau of Longitude has induced the minister to re-establish the observatory of the College of France, which has been important on account of the number of excellent navigators and astronomers its professor has for these thirty

years

years past sent into the world. The zeal and intelligence of M. Vaudoyer, the architect of government, have given a new degree of utility to this observatory; and science is much indebted to him. It was he who arranged the halls of the Institute in *Mazarin* College.

Physique Mecanique, by E. G. Fischer, honorary member of the Berlin Academy of Sciences, and professor of mathematics and physics in the same city: translated from the German, with notes by M. Biot, member of the Imperial Institute of France.

Madame Biot, to whom we owe this translation, has rendered a service to physics. We here find the properties of the movements of solids, fluids, electricity, magnetism, the phænomena of light, the theory of achromatic glasses. M. Biot, who has made some learned notes, complains that the German language is not sufficiently cultivated in France, and that Gehlen's dictionary has not been translated. The excellent inquiries of Volta upon the Galvanic effects of electricity, the work of M. Chladny upon the vibrations of surfaces, was not known until eight years after it was published; and the work of the same author upon stones which fall from the skies was not known among us until the meteoric stone at Aigle attracted the public attention to aërolites. Nevertheless, the reality of the fall of these masses had been long before established from preceding facts, and the force of the reasoning in the work of M. Chladny. I have already complained several times myself that the German language was not sufficiently cultivated in France; and were it not for the Ephemerides of Berlin by M. Bode, and the journal of M. de Zach, we should know nothing at all of the astronomy of Germany. Messrs. Burckhardt and Delambre make us acquainted in the same manner as the *Bibliotheque Britannique* of Geneva acquaints us with what is passing in England.

Theory of the present surface of the earth, or rather impartial inquiries upon the time of the arrangement of the present surface of the earth, founded solely upon facts, without system and without hypothesis; by M. André. It refers every thing to the deluge; but there are interesting observations,

observations, which have no connection with his ideas. In the *Journal des Mines*, no. 108. vol. xviii. pages 321 to 377, they have published observations upon the barometer and upon the heights of mountains, made with incredible labour by this intrepid traveller. This interesting collection for geology and and meteorology forms a continuation to those of sir G. Shuckburgh in the Philosophical Transactions for 1777: it is much to be wished that these examples may be imitated.

M. André, in his Geology of the Mountains he has visited, ascertains every where that the water once flowed over these mountains. He does not endeavour to explain their retreats; but I think I have proved that they are in subterraneous cavities which are under the superficial crust upon which we live. *Journal de Paris*, Nov. 8, 1805; *Journal des Debats*, May 7, 1805.

Noé André was born in 1728; he became a capuchin in 1745. In 1770 he proposed to undertake a chart of Franche-Comté; but, having come to Paris by chance in 1773, he was kindly received by M. Le Monnier, who lodged in the court of the Capuchins: he made some celestial planispheres in 1779 and 1780. In 1781 he set out on his travels, which he prosecuted for six years, at the rate of six months each year. These travels have produced a most valuable collection of observations from the pen of M. André.

M. de Lezenne, professor at Lisle, has printed an elementary *Gnomonique* in 26 pages, which will be very useful to bind up with those astronomical works in which this application is not sufficiently given in detail. I gave in 1784 a more extensive and complete *Gnomonique* in the *Encyclopédie Méthodique,* at the word *Dial*, vol. i. article *Mathematics*. It was intended to introduce it into the fourth volume of my Astronomy, which appeared in 1781; but the abundance of matter in which I have been engaged hindering me from publishing it, it has not hitherto been printed separately, which has limited its usefulness: we there find fourteen species of dials, several of which are not to be found in other treatises, except that of don Bedos, 1778, in

8vo.,

8vo., which is the completest and most extensively known of all.

The necessity of our procuring good instruments without the assistance of the English, has determined government to place pupils with our own most intelligent artists.

The exhibition of our national industry, which took place this year under the auspices of M. Champagny, minister of the interior, has made known a multitude of important subjects which were hitherto unknown. M. Le Noir, one of our best engineers, exhibited an azimuth circle, an equatorial, a repeating circle, a circle of reflection for the navy, a variation compass, an inclination compass: all these instruments made with his own hands. He also exhibited an instrument, by M. Rochon, for reducing distances at sea; a level of a new construction; two circles for finding the horizontal and the vertical angles; a micrometer for measuring the distances of far removed objects; a smaller and more convenient graphometer than any yet used; an armillary sphere, which gives the true time and the mean time; an astronomical ring; a new compensation pendulum made with glass and copper.

M. Lerebours in optics, and M. Jeker in naval instruments, also contributed to this exhibition.

We have distinguished some fine pieces in clock- and watch-work by Messrs. Breguet, Lepaute, Jauvier, Pons, &c.: of these a detail has been given in the catalogue of the exhibition, and in the *Moniteurs* of the 26th of October and 12th of December 1806.

I sent to Rome a circle made by M. Belet, and a pendulum by M. Pons, the precision of which was much admired by the Italian astronomers.

M. Zeichenbach at Munich, and Baumann at Stutgardt, make very fine instruments. With the repeating circles of the latter one may take observations by himself alone, by means of a lead wire placed in the inside of a hollow cylindrical axis, according to the idea of M. Bohnerberger.

M. Mendelssohn, at Berlin, makes sextants with very great exactitude.

The

The emperor having granted some instruments to the Turin observatory, M. Charles Dominique Marie du Chayla has been attached to it. He set out for Turin in September 1806, after having for some time attended the observatory of Paris.

The 22-foot telescope of the observatory has been refitted by M. Caroché, but the stand of it is too embarrassing, and they propose making another, after having expended 50,000 francs for the first: M. Caroché has discovered that it is very difficult to place a mirror weighing 400 pounds in such a way as not to change its form or position when the telescope is pointed at different altitudes.

The 40-foot telescope of Mr. Herschel has not yet furnished the extraordinary results we expected from it. I wrote to him that I was desirous of coming to England to visit this prodigious instrument, as soon as he wrote me that he had no objections: I have not yet received his answer. As Mr. Herschel is now 68 years of age, I am afraid he will not be able to satisfy himself, and that he will not find a successor capable of terminating completely so difficult an enterprise.

[To be continued.]

XVIII. *Problems on the Reduction of Angles. By* T. S. Evans, *F.L.S., of the Royal Military Academy, Woolwich.*

[Continued from p. 34.]

Problem VI.

To find the relation between the angles ASC, FSE, ASF, and CSE, when the plane AE is perpendicular to the triangle ASF, and the figure AE a parallelogram. (Fig. 12.)

By the Problem, p. 29, we have $CS^2 + ES^2 - 2\,CS.\,ES.\,c, CSE = EC^2$ and $AS^2 + FS^2 - 2\,AS.\,FS.\,c, ASF = AF^2$; but as $AF^2 = FC^2$, therefore these two equations are equal to each other. Again, $CS = AS\,/, ASC$, and $ES = FS\,/, FSE$; substituting these for their equals, and putting the two equa-

 tions

tions equal to each other, $\mathrm{AS}^2\,ʃ^2, \mathrm{ASC} + \mathrm{FS}^2\,ʃ^2, \mathrm{FSE} - 2\mathrm{AS.FS.}\,ʃ, \mathrm{ASC}\,ʃ, \mathrm{FSE}\,c, \mathrm{CSE} = \mathrm{AS}^2 + \mathrm{FS}^2 - 2\,\mathrm{AS.FS.}\,c, \mathrm{ASF}$; but it is evident that $\mathrm{AS} : \mathrm{FS} :: \tau, \mathrm{ASC} : \tau, \mathrm{ESF}$; consequently, $\mathrm{FS} = \dfrac{\mathrm{AS}\,\tau, \mathrm{ESF}}{\tau, \mathrm{ASC}}$, which being substituted for its equal in the preceding equation gives us $\mathrm{AS}^2\,ʃ^2, \mathrm{ASC} + \dfrac{\mathrm{AS}^2\,\tau^2, \mathrm{ESF}\,ʃ^2, \mathrm{FSE}}{\tau^2, \mathrm{ASC}} - 2\,\mathrm{AS}^2\,\dfrac{\tau, \mathrm{ESF}\,ʃ, \mathrm{ASC}\,ʃ, \mathrm{FSE}\,c, \mathrm{CSE}}{\tau, \mathrm{ASC}} = \mathrm{AS}^2 + \dfrac{\mathrm{AS}^2\,\tau^2, \mathrm{ESF}}{\tau^2, \mathrm{ASC}} - 2\,\mathrm{AS}^2\,\dfrac{\tau, \mathrm{ESF}\,c, \mathrm{ASF}}{\tau, \mathrm{ASC}}$; which being divided by AS^2, and reduced by observing that $ʃ^2 - 1 = t^2$, and $\tau^2, \times t^2, = 1$, then dividing the whole equation by 2, we get $t^2, \mathrm{ASC} = \tau, \mathrm{ESF}\,ʃ, \mathrm{ASC}\,ʃ, \mathrm{FSE}\,c, \mathrm{CSE}\,\tau, \mathrm{ASC} - \tau, \mathrm{ESF}\,c, \mathrm{ASF}\,t, \mathrm{ASC}$, but $\tau, \mathrm{ESF}\,ʃ, \mathrm{ESF} = \sigma, \mathrm{ESF}$, and $\dfrac{\sigma, \mathrm{ASC}}{t, \mathrm{ASC}} = ʃ, \mathrm{ASC}$; therefore $t, \mathrm{ASC} = \sigma, \mathrm{ESF}\,ʃ, \mathrm{ASC}\,c, \mathrm{CSE} - \tau, \mathrm{ESF}\,c, \mathrm{ASF}$; and by multiplying the whole equation by $s, \mathrm{ESF}\,c, \mathrm{ASC}$, then observing that $\tau, \times s, = c$, and $t, \times c, = s$, we have $s, \mathrm{ASC}\,s, \mathrm{ESF} = c, \mathrm{CSE} - c, \mathrm{ESF}\,c, \mathrm{ASF}\,c, \mathrm{ASC}$, the relation sought.

Hence when the angle CSE, formed at S by two objects at C and E, elevated above the horizon, is given, together with the angles of elevation ASC, FSE, the horizontal angle ASF is easily obtained from this equation:

$$c, \mathrm{ASF} = \frac{c, \mathrm{CSE} - s, \mathrm{ASC}\,s, \mathrm{ESF}}{c, \mathrm{ESF}\,c, \mathrm{ASC}}.$$

But if the other three angles are given to find CSE, we have $c, \mathrm{CSE} = s, \mathrm{ASC}\,s, \mathrm{ESF} + c, \mathrm{ESF}\,c, \mathrm{ASF}\,c, \mathrm{ASC}$.

But in the last problem, supposing SB (fig. 13.) to be perpendicular to AF and DB perpendicular to SB; or, which is the same thing, let the plane BSD be at right angles to the plane AE:

Then by Problem V, $t, \mathrm{ASB}, c, \mathrm{BSD} = t, \mathrm{CSD}$, and $t, \mathrm{FSB}\,c, \mathrm{BSD} = t, \mathrm{ESD}$; the sum of these gives $c, \mathrm{BSD}\,(t, \mathrm{ASB} + t, \mathrm{FSB}) = t, \mathrm{CSD} + t, \mathrm{ESD}$; and since $c, \mathrm{BSD} = \dfrac{t, \mathrm{CSD}}{t, \mathrm{ASB}}$; therefore $t, \mathrm{CSD}\,(t, \mathrm{ASB} + t, \mathrm{FSB})$

$= t,$

$= t, \text{ASB}\ (t, \text{CSD} + t, \text{ESD})$; but by Cagnoli 89, $t, \text{CSD} \times \left(\frac{s, \text{ASF}}{c, \text{ASB}\ c, \text{FSB}}\right) = t, \text{ASB}\ \left(\frac{s, \text{CSE}}{c, \text{CSD}\ c, \text{ESD}}\right)$; or $t, \text{CSD} \times s, \text{ASF}\ c, \text{CSD}\ c, \text{ESD} = t, \text{ASB}\ s, \text{CSE}\ c, \text{ASB}\ c, \text{FSB}$; but $t, \times c, = s$; therefore $s, \text{CSD}\ s, \text{ASF}\ c, \text{ESD} = s, \text{ASB}\ s, \text{CSE}\ c, \text{FSB}$; whence

$$s, \text{ASF} = s, \text{CSE}\ \left(\frac{s, \text{ASB}\ c, \text{FSB}}{s\ \text{CSD}\ c, \text{ESD}}\right);$$

which gives the relation between the six angles ASF, CSE, ASB, FSB, CSD, and ESD.

Cor. 1st. Since $t, \text{BSD} : t, \text{ASC} :: c, \text{ASB} : \text{rad.}$ and
$t, \text{FSE} : t, \text{BSD} :: \text{rad.} : c, \text{BSF}$;
therefore $t, \text{FSE} : t, \text{ASC} :: c, \text{ASB} : c, \text{BSF}$.

Cor. 2d. $t, \text{ASB} : t, \text{CSD} :: \text{rad.} : c, \text{BSD}$ and
$t, \text{FSB} : t, \text{ESD} :: \text{rad.} : c, \text{BSD}$, also
$t, \text{BSD} : t, \text{ASC} :: \text{rad.} : c, \text{ASB}$ and
$t, \text{BSD} : t, \text{FSE} :: \text{rad.} : c, \text{FSB}$, whence
$t, \text{ASC} : c, \text{ASB} :: t, \text{FSE} : c, \text{FSB}$ and
$t, \text{CSD} : t, \text{ASB} :: t, \text{ESD} : t, \text{FSB}$; therefore
by multiplying the two last proportions together, we have
$t, \text{ASC}\ t, \text{CSD} : t, \text{ASB}\ c, \text{ASB} :: t, \text{FSE}\ t, \text{ESD} : t, \text{FSB}\ c, \text{FSB}$
or, $t, \text{ASC}\ t, \text{CSD} : s, \text{ASB} :: t, \text{FSE}\ t, \text{ESD} : s, \text{FSB}$.

Cor. 3d. From preceding cor.
$t, \text{FSB}\ t, \text{CSD} = t, \text{ESD}\ t, \text{ASB}$; therefore
$t, \text{FSB} : t, \text{ASB} :: t, \text{ESD} : t, \text{CSD}$.

But when the plane SBD, that is perpendicular to AE passing through S, falls without the figure, we have (fig. 14.) $t, \text{CSD} - t, \text{ESD} : t, \text{ASB} - t, \text{FSB} :: c, \text{BSD} : \text{radius}$; but by Cagnoli, art. 91,

$$t, \text{CSD} - t, \text{ESD} = \frac{s, (\text{CSD} - \text{ESD})}{c, \text{CSD}\ c, \text{ESD}};\ \text{therefore}$$

$$\frac{s, (\text{CSD} - \text{ESD})}{c, \text{CSD}\ c, \text{ESD}} : \frac{s, (\text{ASB} - \text{FSB})}{c, \text{ASB}\ c, \text{FSB}} :: c, \text{BSD} : \text{rad. or,}$$

$$s, \text{ASF} = \frac{s, \text{CSE}\ c, \text{ASB}\ c, \text{FSB}}{c, \text{CSD}\ c, \text{ESD}\ c, \text{BSD}}.$$

Suppose now the line EF to move up to BD and to coincide with it, then the equation becomes $s, \text{ASB} = \frac{s, \text{CSD}\ c, \text{ASB}}{c, \text{CSD}\ c, \text{BSD}}$, or $\frac{s, \text{ASB}}{c, \text{ASB}} \times c, \text{BSD} = \frac{s, \text{CSD}}{c, \text{CSD}}$, but the $\frac{\text{sine}}{\text{cos.}}$ = tang.;

= tang.; therefore it becomes $t, ASB\, c, BSD = t, CSD$; agreeing with the equation before found for this case, p. 34.

Problem VII.*

To find the relation between the angles ASC, BSE, ASB, and CSE, when the point E is higher than C. (Fig. 15.)

First we have $AB = SB\, t, ASB$; $BD = SB\, t, BSD$; $BE = SB\, t, BSE$ and $AC = AS\, t, ASC = SB\, \int, ASB\, t, ASC = BD = SB\, t, BSD$, also $DE = SB\, (t, BSE - t, BSD)$; but $DE^2 + DC^2 = EC^2 = SB^2\, (t, BSE - t, BSD)^2 + SB^2\, t^2, ASB = SB^2\, (\overline{t, BSE - t, BSD}|^2 + t^2, ASB)$, and $CS = AS\, \int, ASC = SB\, \int, ASB\, \int, ASC$; also $SE = BS\, \int, BSE$; then by trig. $c, CSE = \dfrac{CS^2 + ES - CE^2}{2\,CS.\,ES} =$

$$\frac{(SB\, \int, ASB\, \int, ASC)^2 + (BS^2 \int^2, BSE) - SB^2\, (\overline{t, BSE - t, BSD}|^2 + t^2, ASB)}{2\, SB\, \int, ASB\, \int, ASC \times SB\, \int, BSE}$$

$$= \frac{\int^2, ASB\, \int^2, ASC + \int^2, BSE - t^2, BSE + 2t, BSE\, t, BSD - t^2, BSD - t^2, ASB}{2\int, ASB\, \int, ASC\, \int, BSE}, \text{or}$$

$\int^2, ASB\, \int^2, ASC + 1 + 2t, BSE\, t, BSD - t^2, BSD - t^2, ASB = c, CSE \times 2\int, ASB\, \int, ASC\, \int, BSE$; but by Cor. to Problem V, $t, BSD = \int, ASB\, t, ASC$; consequently $\int^2, ASB\, \int^2, ASC + 2\, t, BSE\, \int, ASB\, t, ASC - \int^2, ASB\, t^2, ASC - t^2, ASB = c, CSE \times 2\int, ASB\, \int, ASC\, \int, BSE - 1$; but $\int^2 - t^2 = \text{rad.}^2$; whence by reducing and dividing the whole equation by 2, we get $c, CSE\, \int, ASC\, \int, BSE - c, ASB = t, BSE\, t, ASC$; which divided by $\int, ASC\, \int, BSE$, becomes $c, CSE - c, ASB\, c, ASC\, c, BSE = t, BSE\, t, ASC\, c, BSE\, c, ASC$; and since $t, \times c, = s$, therefore it becomes $c, CSE - c, ASB\, c, ASC\, c, BSE = s, BSE\, s, ASC$, for the required relation.

* This problem may thus be solved by spherics:

Let Z (fig. 16.) be the zenith of the observer at the station S, then the arc ZD will be the complement of the angle CSA of the object AC, and ZF the complement of the angle of elevation ESB of the object EB taken at the point S; but by what is shown, p. 33, the angle DZF = the angle ASB. Now in the triangle DZF we have given by observation the three sides to find the angle DZF, which may be had by either of the following formulæ. (Cagnoli, 463, 464.)

$$s, \tfrac{1}{2}\, DZF = \sqrt{\frac{s, (\frac{1}{2} S - ZD)\; s, (\frac{1}{2} S - ZF)}{s, ZD\; s, ZF}}$$

$$c, \tfrac{1}{2}\, DZF = \sqrt{\frac{s, \frac{1}{2} S\; s, (\frac{1}{2} S - DF)}{s, ZD\; s, ZF}}$$

where S is put to represent the sum of the sides.

relation. We should have obtained the same equation if we had not considered SB perpendicular to AB and BE.

Hence $c, \mathrm{ASB} = \dfrac{c, \mathrm{CSE} - s, \mathrm{BSE}\ s, \mathrm{ASC}}{c, \mathrm{ASC}\ c, \mathrm{BSE}}$, and $c, \mathrm{CSE} =$ $s, \mathrm{BSE}\ s, \mathrm{ASC} + c, \mathrm{ASB}\ c, \mathrm{ASC}\ c, \mathrm{BSE}$.
Differing from the equation, p. 130, only in the letters.

Problem VIII.*

To find the relation between the angles DAC, DBC, ACB, and ADB, when the plane ABC is perpendicular to the line DC. (Fig. 17.)

First we have $\mathrm{AD} = \mathrm{CD}\ ſ, \mathrm{ADC}$; $\mathrm{BD} = \mathrm{CD}\ ſ, \mathrm{BDC}$, $\mathrm{AC} = \mathrm{CD}\ t, \mathrm{ADC}$; $\mathrm{BC} = \mathrm{CD}\ t, \mathrm{BDC}$; and by trigonometry $\mathrm{BA}^2 = \mathrm{BD}^2 + \mathrm{AD}^2 - 2\,\mathrm{BD}.\mathrm{AD}.\ c, \mathrm{BDA} = \mathrm{BC}^2 + \mathrm{AC}^2 - 2\,\mathrm{BC}.\mathrm{AC}.\ c, \mathrm{BCA}$; or by substituting the above values of these quantities $\mathrm{CD}^2 ſ^2, \mathrm{BDC} + \mathrm{CD}^2 ſ^2, \mathrm{ADC} - 2\,\mathrm{CD}^2\ ſ, \mathrm{BDC}\ ſ, \mathrm{ADC}\ c, \mathrm{BDA} = \mathrm{CD}^2\ t^2, \mathrm{BDC} + \mathrm{CD}^2\ t^2, \mathrm{ADC} - 2\,\mathrm{CD}^2\ t, \mathrm{BDC}\ t, \mathrm{ADC}\ c, \mathrm{BCA}$; which divided by CD^2 and reduced, remembering that $ſ^2 - t^2 = \mathrm{rad.}^2$; then dividing the whole by 2, it becomes $ſ, \mathrm{BDC}\ ſ, \mathrm{ADC}\ c, \mathrm{BDA} - 1 = t, \mathrm{BDC}\ t, \mathrm{ADC}\ c, \mathrm{BCA}$; and by dividing the whole by $ſ, \mathrm{BDC}\,ſ, \mathrm{ADC}$, we get $c, \mathrm{BDA} - c, \mathrm{BDC}\ c, \mathrm{ADC} = s, \mathrm{BDC}\ s, \mathrm{ADC}\ c, \mathrm{BCA}$; which gives the relation between the angles.

Hence $c, \mathrm{BCA} = \dfrac{c, \mathrm{BDA} - c, \mathrm{BDC}\ c, \mathrm{ADC}}{s, \mathrm{BDC}\ s, \mathrm{ADC}}$, and c, BDA $= s, \mathrm{BDC}\ s, \mathrm{ADC}\ c, \mathrm{BCA} + c, \mathrm{BDC}\ c, \mathrm{ADC}$.

Problem IX.

To find the relation between the angles BCD, BDC, ACD, ADC, ADB, and ACB, when the plane ACB is not perpendicular to the line CD. (Fig. 18.)

By trigonometry $\mathrm{CA} = \dfrac{\mathrm{CD}\ s, \mathrm{ADC}}{s, \mathrm{CAD}}$, $\mathrm{DA} = \dfrac{\mathrm{CD}\ s, \mathrm{DCA}}{s, \mathrm{CAD}}$

* This Problem may thus be solved by spherics:

With the radius DC (fig. 17.) describe the arcs CE, EF, FC, and the lines AC, BC being tangents to the arcs EC, FC, the angle formed by these tangents and arcs at the point C are equal (p. 33). Now in the spheric triangle FCE we have the three sides given to find the angle FCE, which is equal to the angle ACB, and may be found by either of the theorems on page 132.

$CB = \frac{CD\, s, BDC}{s, CBD}$ and $DB = \frac{CD\, s, DCB}{s, CBD}$; also since $BA^2 = BD^2 + AD^2 - 2\,BD.\,AD.\,c, ADB = BC^2 + AC^2 - 2\,BC.\,AC, c, ACB$; therefore by substituting the above values of these quantities we get $\frac{CD^2\, s^2, DCB}{s^2, CBD} + \frac{CD^2, s^2, DCA}{s^2\, CAD} - \frac{CD^2\, s, DCB\, s, DCA\, c, ADB}{s, CBD\, s, CAD} = \frac{CD^2\, s^2. BDC}{s^2, CBD} + \frac{CD^2\, s^2, ADC}{s^2, CAD} - \frac{2\,CD^2\, s, BDC\, s, ADC\, c, ACB}{s, CBD\, s, CAD}$; but the angles CBD, CAD being the supplements of (CBD + CDB) and (ACD + ADC) respectively, let them be substituted for them, and then dividing the whole equation by CD^2, we have $\frac{s^2, DCB}{s^2, (BDC + BCD)} + \frac{s^2, DCA}{s^2, (ADC + ACD)} - \frac{2\, s, DCB\, s, DCA\, c, ADB}{s, (BDC + BCD)\, s, (ADC + ACD)} = \frac{s^2, BDC}{s^2, (BDC + BCD)} + \frac{s^2, ADC}{s^2, (ADC + ACD)} - \frac{2\, s, BDC\, s, ADC\, c, ACB}{s, (BDC + BCD)\, s, (ADC + ACD)}$ which cleared of fractions becomes s^2, DCB s^2, (ACD + ADC) + s^2, DCA s^2, (BDC + BCD) − 2 s, DCB s, DCA c, ADB s, (BDC + BCD) s, (ADC + ACD) = s^2, BDC s^2, (ACD + ADC) + s, ADC s^2, (BDC + BCD) − 2 s, BDC s, ADC c, ACB s, (BDC + BCD) s, (ADC + ACD); or (s^2, DCB − s^2, BDC) s^2, (ACD + ADC) + (s^2, DCA − s^2, ADC) s^2, (BDC + BCD) = [2 s, DCB s, DCA c, ADB − 2 s, BDC s, ADC c, ACB] s, (BDC + BCD) s, (ADC + ACD); but by art. 84, Cagnoli's Trigonometry; s^2, DCB − s^2, BDC = s, (BCD + BDC) s, (BCD − BDC); whence by substitution we get s, (BCD + BDC) s, (BCD − BDC) s^2, (ACD + ADC) + s, (ACD + ADC) s, (ACD − ADC) s^2, (BDC + BCD) = [2 s, DCB s, DCA c, ADB − 2 s, BDC s, ADC c, ACB] × s, (BDC + BCD) s, (ADC + ACD), and dividing the whole equation by s, (BCD + BDC) × s, (ADC + ACD) it becomes s, (BCD − BDC) s, (ACD + ADC) + s, (ACD − ADC) s, (BDC + BCD) = 2 s, DCB s, DCA c, ADB − 2 s, BDC s, ADC c, ACB; but by Cagnoli, art. 52, s, (BCD − BDC) = s, BCD c, BDC − c, BCD s, BDC, and s, (BDC + BCD) = s, BDC c, BCD + c, BDC s, BCD: by substituting these for their equals, then actually multiplying them together, and dividing the whole equation by 2, we have s, BCD

c, BDC

c, BDC s, ACD c, ADC $-$ c, BCD s, BDC c, ACD s, ADC $=$ s, DCB s, DCA c, ADB $-$ s, BDC s, ADC c, ACB, which gives the relation betwixt the six angles at C and D.

Suppose now BC to be perpendicular to CD, then the s, BCD $= 1$ and c, BCD $= 0$; consequently, by making these alterations the equation becomes c, BDC s, ACD c, ADC $=$ s, DCA c, ADB $-$ s, BDC s, ADC c, ACB.

In the same manner an equation may be found when AC is perpendicular to CD, as follows: s, BCD c, BDC c, ADC $=$ s, DCB c, ADB $-$ s, BDC s, ADC c, ACB.

If both AC and BC be conceived perpendicular to CD, then s, ACD $=$ s, BCD $= 1$, and c, ACD $=$ c, BCD $= 0$; which alteration being made, we have c, BDC c, ADC $=$ c, ADB $-$ s, BDC s, ADC c, ACB; agreeing exactly with the equation before found for this case, Problem VIII.

When the angle BDC $=$ ADC, then s, BCD c^2, BDC s, ACD $-$ c, BCD s^2, BDC c, ACD $=$ s, DCB s, DCA c, ADB $-$ s^2, BDC c, ACB.

If at the same time AC and BC be both conceived perpendicular to CD, we have c^2, BDC $=$ c, ADB $-$ s^2, BDC c, ACB.

And if the angles BDA, BCA, be made equal to nothing by the side AC moving up to BC and coinciding with it, then c^2, BDC $=$ rad.^{2} $-$ s^2, BDC; which is the well known property of a right-angled triangle.

If AC be supposed perpendicular to BC, at the same time that AC and BC are both perpendicular to CD; then c, ACB being equal to 0, we get c, BDC c, ADC $=$ c, ADB; which is the equation given for this case in Problem IV, p. 34.

From the general equation we get c, ACB $=$ s, DCB s, DCA c, ADB σ, BDC σ, ADC $+$ c, BCD c, ACD $-$ s, BCD s, ACD τ, BDC τ, ADC, and c, BDA $=$ c, ACB s, BDC s, ADC σ, DCB σ, DCA $+$ c, BDC c, ADC $-$ τ, BCD τ, ACD s, BDC s, ADC; which equations give the angles ACB or ADB when the five other angles are known.

By a little attention to the solutions of these problems we shall perceive that they proceed upon the same elementary principles as spheric trigonometry: for in that science one

 convenient

convenient line is considered as the radius, and then the relation subsisting between it and the rest of the lines of the figure in terms of this radius is found out by means of plane trigonometry; after which, if the radius be made equal to unity, all the others become merely the trigonometric lines of our common tables. This has been the method of proceeding in solving the foregoing problems; and hence the reason why no linear dimensions are retained in the results. Thus in solving Problem VIII, the side CD has been made the radius, and all the other lines of the figure have been found in terms of it; but as the equation was throughout equally affected by CD, we put it equal to unity, and the whole is immediately referred to a sphere, having its radius equal to 1.

It will be found that the next problem to this, viz. the IXth, contains all the fundamental principles of spheric trigonometry.

If the earth be considered as a sphere, the three angles of a triangle on its surface, being reduced to the same distance from the centre, will represent those of a spheric triangle, the planes of whose three sides pass through the centre of the earth; therefore when the proper reductions have been applied to the observed angles for this purpose by means of the foregoing problems, the resolution of the triangle becomes extremely easy by spheric trigonometry.

As radii drawn from the centre of a sphere to the circumference, or beyond, continually diverge or recede from each other, therefore when a chain of triangles are observed near the earth's surface, it is clear they must be reduced so as to have all their angular points at the same distance from the earth's centre, when considered as a sphere; but if the earth's figure be taken as a spheroid of revolution, these triangles are commonly reduced to the level of the sea in the same latitude.

The three angles of each triangle are then added together; and since this triangle is in fact spherical, therefore the sum of the angles must necessarily surpass 180°. This excess, which is also affected by the error of observation, is to be divided by 3, and one-third part is to be taken from each

each angle, which makes their sum, thus corrected, equal to 180° exactly: with these the distances are computed, considering the triangle merely as a plane one.

When the sides of a spheric triangle are very small in comparison of the radius, which is the case in most geodesic operations, there are some convenient formulæ in Legendre's Trigonometry, p. 463, for its solution. Another way is to take the chords of the arcs and find the angles formed by those chords at each of the stations, and the triangle then to be computed is actually a plane one. On this subject see the xviiith chapter of Cagnoli's Trigonometry *.

As all substances expand by heat and contract by cold, if the measurement of the base has been conducted under different temperatures, it will require reducing to that length which it would have been found had the temperature remained uniform during the whole time of measuring.

The most advantageous shape of a triangle for determining the angles is, when the triangle is equilateral. (Cagnoli, 332.) We must therefore in practice approach as near to that as circumstances will admit.

Persons unaccustomed to the practice would naturally suppose that the small errors in a chain of triangles † would accumulate to such a degree as to render the work very uncertain after passing through the first six or eight triangles: however, the contrary is known to be the fact. The small errors compensate each other, and the work may be depended upon with as much certainty after twenty triangles as after two. Colonel Mudge, the present able conductor of the trigonometrical survey in England, from the base measured on Hounslow Heath, carried on a series of 24 triangles over a distance of 60 miles; and then, having measured a base of verification, and compared it with the computed triangles, had the satisfaction to find a difference of no more than 4½ inches between them. This remarkably near agreement, whilst it proves that the errors compensate each other,

* This excellent work has been translated into English by the author of this memoir, and is now ready for the press.

† Snell appears to have been the first who measured an arc of the meridian by a chain of triangles.

shows

shows also the great care that he took in measuring the angles, and in conducting the whole of the operations.

It cannot be too strongly recommended to observers to measure all three of the angles, several times, on different parts of the arc of the instrument, and then to take a mean of them all. If time will permit, this should be repeated for several successive days, at all hours of the day; for it has been strongly suspected that there exists a lateral as well as a horizontal refraction.

It sometimes happens that the sun illuminates only one-half of an object; and the observer, if he is not on his guard, will bisect the dark part of the object instead of taking its centre. This may be avoided by waiting till the sun is obscured, or causes an equal illumination on both sides of it; or by choosing, if possible, such objects as are projected on the sky by rays drawn from the observer's eye; then the object appearing dark, and the sky behind it being light, he cannot fail of bisecting it properly with a little attention.

The height of a signal is of more consequence than its width in geodesic operations; for it is evident, the wider it is, the more uncertainty there will be in bisecting it. Delambre took $\frac{3}{20000}$ of the greatest distance for the height of his signals, whence its height appeared under an angle of $31''$. Or if $D =$ the greatest distance of a signal, $H =$ its required height; then he made $H = D\ s, 31'' = 0{\cdot}00015\ D$, and the base he made one-third of the height.

Care should be taken that the sides of the triangles are not chosen too long or too short in comparison of the length of the base. A side should never subtend a less angle than 25° on any account. When the chain of triangles is long between the first base and the base of verification, the sides of the triangles may be so chosen as to increase in length gradually till about the middle, and then to decrease in the same manner from the middle to the base of verification. By this means one or two triangles may be saved without sensibly diminishing the accuracy; and the advantage will be, that so much trouble and computation will be saved.

The strictest attention possible should be paid to the ad-

justment

justment of the instrument before each observation; and no angles should be measured when the wind is high, and the stand or scaffolding not very firm *.

Legendre has proved, in a work lately published by him, entitled "*Nouvelle Méthode pour la Détermination des Orbites des Comètes*," that in order to diminish as much as possible the influence of errors committed in measuring arcs, or in any series of observations whatever, we must render the sum of the squares of these errors a minimum. He has also proved, in another work, that the difference between spheric and spheroidic angles does not amount to so much as the $\frac{1}{500}$th part of a second in the greatest of the French triangles. The consideration of the spheroid may therefore with safety be neglected, and the earth may be taken as a perfect sphere in all such geodesic operations as are usually carried on.

The real places of the stations cannot be assigned by only knowing their distances; it it necessary, besides, that we should know the latitude and longitude of one place, and the angle made by one of the sides of the triangle with the meridian of this place: we may then refer the other two places to the meridian and equator, or determine their latitudes and longitudes.

In taking a series of triangles for any purpose, such as determining an arc of the meridian, it is necessary to measure either one or two bases of verification, in order to ascertain whether the results obtained from the calculation are correct at the end of the operations.

* It frequently happens that exhalations, and other causes, will make the object move about in the field of view of the telescope: when this is the case, the observation had better always be deferred till the object appears steady.

XIX. *On*

XIX. *On the Stanhope Temperament of the Musical Scale.* *By Mr.* John Farey.

To Mr. Tilloch.

Sir,

Having undertaken to elucidate the Stanhope scale of musical intervals by every means in my power, I beg here to resume the tables, pages 195 to 197 of your last volume, for the purpose of stating the Stanhope intervals, &c. in two other modes of notation: the first of which, or that by *tone major* T $(\frac{8}{9})$, *tone minor* t $(\frac{9}{10})$, and *hemitone* H $(\frac{15}{16})$, is already well known, and was omitted on the above occasion merely for want of room. Respecting the second mode, or that by the *Schisma* Σ $\left(\frac{2^{15}}{3^8\,5}\right)$, the *lesser Fraction* f $\left(\frac{3^{37}}{2^{54}5^2}\right)$, and the *most Minute* m $\left(\frac{3^{84}\,5^{12}}{2^{161}}\right)$, I wish to remark, that the interval Σ I have been long acquainted with, as the *lesser Diesis* of Mr. Maxwell, and it is the flat temperament of the fifths in my equal-temperament, mentioned in your last number, p. 65: the interval m, is the wolf or difference between 12 fifths tempered as there directed and 7 octaves: the other interval f I obtained, from a valuable set of manuscripts by the late Mr. *Marmaduke Overend*, organist of Isleworth, in Middlesex, now in the possession of Dr. Callcott, who obligingly lent them to me for perusal, since I had completed my equal temperament. The intervals m and f are the two smallest intervals which Mr. *Overend* was able to deduce, from an almost endless subtraction of the ratios expressing musical intervals from each other, in all their forms of combination; their smallness, compared even with Σ, seemed to me to render them fit to be used therewith in expressing musical intervals; and so I have found them, in an extensive reduction of Mr. *Overend's* scales of intervals into this notation, and also in Lord *Stanhope's* intervals, shown in columns 6 and 7 of Table I, and columns 4 and 5 of Table II. following.

Table

Table I. *Showing the Relations which the several Notes in an Octave bear to the fundamental Note C, when tuned according to the Stanhope Temperament.*—See vol. xxvii. p. 195.

1.	2.	3.	4.	5.	6.	7.
12	c	$\frac{1}{2}$	VIII	$3T + 2t + 2H$	$612\Sigma + 12f + 53m$	
11	B		VII	$3T + 2t + H$	$555\Sigma + 11f + 48m$	
10	♭B		7th	$2T + 2t + 2H$	$508\Sigma + 10f + 44m$	
9	A	$\frac{3}{5}$	VI	$2\frac{1}{3}T + 1\frac{2}{3}t + H$	$454\frac{2}{3}\Sigma + 9f + 39\frac{1}{3}m$	$+ 3\frac{2}{3}\Sigma + \frac{1}{3}m$
8	♭A	$\frac{5}{8}$	6th	$2T + 1\frac{1}{2}t + H$	$404\frac{1}{2}\Sigma + 8f + 35m$	$- 10\frac{1}{2}\Sigma - m$
7	G	$\frac{2}{3}$	V	$2T + t + H$	$358\Sigma + 7f + 31m$	
6	♭G		IV	$T + 1\frac{1}{2}t + H$	$300\frac{1}{2}\Sigma + 6f + 26m$	
5	F	$\frac{3}{4}$	4th	$T + t + H$	$254\Sigma + 5f + 22m$	
4	E	$\frac{4}{5}$	III	$T + t$	$197\Sigma + 4f + 17m$	
3	♭E	$\frac{5}{6}$	3d	$T + \frac{1}{2}t$	$150\frac{1}{2}\Sigma + 3f + 13m$	$- 10\frac{1}{2}\Sigma - m$
2	D		II	$\frac{2}{3}T + \frac{1}{3}t$	$100\frac{1}{3}\Sigma + 2f + 8\frac{2}{3}m$	
1	♭D		2d	$\frac{1}{2}t$	$46\frac{1}{2}\Sigma + f + 4m$	
	C		Key			
Intervals in half notes.	Letters.	Ratios.	Marks.	Diatonic Notation.	A new Notation.	Differences from the Diatonic in new Notation.
		Diatonic		Stanhope Intervals expressed in		

Table II. *Showing the Relations of several Notes or Intervals in the Stanhope Temperament, &c. to the Key Note* C.—See vol. xxvii. p. 196 and 197.

No.	2.	3.	4.	5.
1	*True major Sixth*, VI, $\frac{3}{5}$ - -	$2T + 2t + H$	$451\Sigma + 9f + 39m$	
2	*True minor Sixth*, 6th, $\frac{5}{8}$ - -	$2T + t + 2H$	$415\Sigma + 8f + 36m$	
3	Stanhope Vths, B♭G, and ♭E ♭B	$T + 1\frac{1}{2}t + 2H$	$357\frac{1}{2}\Sigma + 7f + 31m$	$-\frac{1}{2}\Sigma$
4	*Equal temperament* V - -	$\frac{7}{4}T + \frac{7}{6}t + \frac{7}{6}H$	$357\Sigma + 7f + 30\frac{11}{12}m$	$-1\frac{1}{2}\Sigma - \frac{1}{12}m$
5	*Tri-equal quint* - - -	$1\frac{2}{3}T + 1\frac{1}{3}t + H$	$354\frac{1}{3}\Sigma + 7f + 30\frac{2}{3}m$	$-3\frac{2}{3}\Sigma - \frac{1}{3}m$
6	*Bi-equal third* - - -	$T + \frac{1}{2}t + H$	$207\frac{1}{2}\Sigma + 4f + 18m$	$+10\frac{1}{2}\Sigma + m$
7	Stanhope IIId, ♭BD - -	$1\frac{2}{3}T + \frac{1}{3}t$	$204\frac{1}{3}\Sigma + 4f + 17\frac{2}{3}m$	$+7\frac{1}{3}\Sigma + \frac{2}{3}m$
8	*Equal temperament* III - -	$T + \frac{2}{3}t + \frac{2}{3}H$	$204\Sigma + 4f + 17\frac{2}{3}m$	$+7\Sigma + \frac{2}{3}m$
9	Stanhope IIId, A♭D - -	$\frac{2}{3}T + \frac{5}{6}t + H$	$203\frac{5}{6}\Sigma + 4f + 17\frac{2}{3}m$	$+6\frac{5}{6}\Sigma + \frac{2}{3}m$
10	Stanhope IIId, FA - - -	$1\frac{1}{3}T + \frac{2}{3}t$	$200\frac{2}{3}\Sigma + 4f + 17\frac{1}{3}m$	$+3\frac{2}{3}\Sigma + \frac{1}{3}m$
11	Stanhope IIId, D♭G - -	$\frac{1}{3}T + \frac{7}{6}t + H$	$200\frac{1}{6}\Sigma + 4f + 17\frac{1}{3}m$	$+3\frac{1}{6}\Sigma + \frac{1}{3}m$
12	*True minor Third*, 3d, $\frac{5}{6}$ -	$T + H$	$161\Sigma + 3f + 14m$	
13	*Sixiéme Wolf* - - -	$-T + 2t - 2H$	$32\Sigma + 3m$	
14	*Tierce Wolf* - - -	$-t + 2H$	$21\Sigma + 2m$	
15	*Quint Wolf* - - -	$3T - 2t - 2H$	$12\Sigma + m$	
16	Elementary *comma*, c - -	$T - t$	$11\Sigma + m$	
17	*Key note*, C			
No.	Names of Intervals.	Diatonic Notation.	A new Notation.	Differences from the Diatonic in new Notation.
		Stanhope Intervals, &c. expressed in		

For explaining these and other musical terms, which have occurred in a very careful perusal of Mr. Overend's manuscripts, and Dr. Callcott's additions therein, I have enclosed a copy of a table* (see Plate V.) that I drew up to assist me in reading and comparing them, which may prove acceptable to your musical readers in general, and contribute perhaps towards fixing the nomenclature of this science. I need now only add that f = $\cdot 149661 \times \Sigma$, and m = $\cdot 0078631 \times \Sigma$; these numbers being decimals of Σ, or logarithms of a particular kind, wherein the index 1 is equal to Σ.—I am

Your obedient servant,

12, Upper Crown-street, Westminster, July 2, 1807. JOHN FAREY.

XX. *A Letter to His Royal Highness the Duke of Cumberland, from Dr.* J. W. CALLCOTT, *respecting the Stanhope Temperament: with a Letter from Earl* STANHOPE *to Dr.* CALLCOTT *upon that Subject.*

[In compliance with a particular request from Dr. *Callcott* and Mr. *Farey* I give a place to the following correspondence, which has already appeared in print. It will be read with interest by those who are versant in the theory of music, which seems now very much to engross the attention of the public. —A. T.]

To His Royal Highness the Duke of Cumberland.

May it please your Royal Highness,

SINCE Your Royal Highness is so great a lover of Music, and takes so great an interest in the new Temperament of the Piano Forte, which is now so much the subject of conversation in the Musical World, I hope that the present liberty will be excused.

Earl STANHOPE has just sent me a Letter on his System, which will throw great light on the *mathematical* part of the subject, in answer to that contained in Mr. TILLOCH's Philosophical Magazine of April last, by Mr. JOHN FAREY.

* As it would be difficult to get this curious table executed correctly in letter-press, I shall cause it to be engraven, and given with the next or a succeeding number.—A. T.

The

The following Letter will clearly show what a triumphant answer Earl STANHOPE has made to those futile observations.—I have the honour to be,

With the greatest respect,
Your Royal Highness's most obedient
And most humble servant,
J. W. CALLCOTT.

Kensington Gravel Pits,
June 15, 1807.

Letter from Earl STANHOPE *to Dr.* CALLCOTT.

SIR, Stratford Place, June 13, 1807.

YOU ask me my opinion of Mr. JOHN FAREY's observations "*On the Stanhope Temperament of the Musical Scale.*" The mistakes which he makes are so extremely obvious, that they are scarcely worth your attention.

In the first place, he begins by objecting to my having divided the Scale of my Monochord, into *one hundred and twenty parts*; and he wishes to substitute a division of *one hundred parts*.

I chose that number, 120, on purpose; because it is divisible *exactly* into *two thirds*, by the number 80; into *three quarters*, by the number 90; into *four fifths*, by the number 96; into *eight fifteenths*, by the number 64; and into *halves*, by the number 60. So that, if the length of the C wire be made equal to 120 equal divisions on the Scale, the length of the E wire will be *exactly* 96 of those divisions, according to my Temperament; that of the F wire will be *exactly* 90; that of the G wire, will be *exactly* 80; that of the B wire, will be *exactly* 64; and that of the C wire next above, will be *exactly* 60.

Whereas, Mr. FAREY, by injudiciously dividing his scale into 100 equal parts, instead of the proper number, which is 120, cannot express those important lengths *in round numbers*; but he is obliged to express the length of the G wire, by the number 66 on his Scale, followed by the decimal fraction 66666 *ad infinitum*. In like manner he expresses the length of the B wire by the number 53 on his Scale,

Scale, followed by the decimal fraction 33333 *ad infinitum*. And this alteration he considers as an improvement!

He misconceives (for I do not impute to him an intention wilfully to misrepresent) what I have said respecting "*the value*" of the C Wolf, and "*the value*" of the Quint Wolf, in my Treatise. Any person, however, who reads my work with common attention, will clearly perceive that *those deviations from perfect intervals* are concisely, as well as accurately and conveniently, expressed by me by means of the difference between the *lengths* of certain wires respectively. And he will not, as a man of science, venture to assert, that those respective *lengths* are not accurately calculated, or that the *difference* between them is not accurately stated. And it is by means of the relative and accurate *lengths of wires*, which are of the same quality and thickness, and which have precisely the same degree of tension, that I have, throughout my whole work, endeavoured to explain, to an enlightened and intelligent public, those interesting truths, which I wished to make them clearly understand, upon a subject which the ablest men belonging to the musical profession justly consider "*as the very basis and foundation of musical science.*" For, when a man talks of a *Quint*, or of any other musical interval, in any key whatsoever, he must have in view some given mode of *temperament*; or else he does not conceive with clearness, nor express himself with precision.

Mr. Farey, in noticing what I have said in my pamphlet respecting the *Wolves*, expresses himself thus:—"His Lordship speaks of *four* Wolves in the *Major Thirds*, whereas" (says Mr. Farey) "there is but *one* such interval, which for distinction I have called the *Tierce Wolf*, and shown its value in No. 14 of Table II; where it must be evident that this interval, VIII — 3III, owing to the *equality* of all the octaves, whether taken above C, G, D, A, E, or any other of the 12 Notes, is always of *the same value*, and is no more the C Wolf than that of any other letter in the Gamut: what could have induced his Lordship to limit his inquiries respecting the *Major Thirds* in his Essay, to the five columns, is to me a mystery."

 This

This expression of FIVE columns, does alone prove to a demonstration, that Mr. FAREY does not understand even the first principles of my new mode of tuning instruments with fixed tones. For, in my Treatise on that subject (in pages 7, 8, and 9), I express myself as follows:

"There are FOUR OTHER WOLVES in the major thirds. But, in order to explain this part of the subject, it will be necessary first to show that there are four series of major thirds which are *unalterably distinct* from each other. Those four series are shown, in four columns, in the following TABLE OF SUCCESSIVE MAJOR THIRDS, where the four keys C, G, D, A, are placed, in the lowest line, in the order of occurrence as successive quints. The other keys are then to be placed, as major thirds, in the regular order shown in the TABLE.

TABLE OF SUCCESSIVE MAJOR THIRDS. (*Read each column from the bottom upwards.*) First, or, C column.	Second, or G column.	Third, or D column.	Fourth, or A column.	Higher octaves of the keys in the first column.
Middle C.	Middle G.	First treble D.	First treble A.	Second treble E.
First bass A flat.	Middle E flat.	Middle B flat.	First treble F.	Second treble C.
First bass E.	First bass B.	Middle G flat.	First treble D flat.	First treble A flat.
First bass C; or key-note.	First bass G.	Middle D.	Middle A.	First treble E.

"By inspecting the above simple TABLE, it will clearly appear that the following series of major thirds, viz. C, E;—E, G sharp, which is the same key as A flat;—and A flat, C, forms a column, in which those three successive major thirds return in constant and regular succession, without ever including in that series any of the nine major thirds. So that, Nature has, as it were (if I may be allowed the expression), *imprisoned* that series of three major thirds in a column by itself.

"The

"The series of the three major thirds, G, B;—B, D sharp, which is the same key as E flat;—and E flat, G, forms a second column.

"The series of the three major thirds, D, F sharp, which is the same key as G flat;—G flat, B flat;—and B flat, D, forms a third column.

"And, in like manner, the series of the remaining three major thirds, A, C sharp, which is the same key as D flat; —D flat, F;—and F, A, forms a fourth column.

"In order to distinguish these four columns from each other, I shall name them from the lowest key in each column respectively; thus we shall have,

"1st, The C column, which consists of C, E, and A flat.

"2dly, The G column, which consists of G, B, and E flat.

"3dly, The D column, which consists of D, G flat, and B flat.

"4thly, The A column, which consists of A, D flat, and F.

"Now it is a very curious fact, that each of these four columns of major thirds has ITS OWN DISTINCT WOLF, exclusively of the QUINT WOLF, which, as we have already seen, affects all the twelve keys without exception.

"If, in the C column, for example, the three successive major thirds, C, E;—E, G sharp, which is the same key as A flat;—and A flat, C, be all made quite perfect; then C C, which is thus produced by means of those three perfect thirds, will not be a perfect octave, but it will be *flatter* than the perfect octave C C would be. *The difference of pitch, between the C derived from the major thirds and the C octave corresponding to it, is what I shall call* THE C WOLF. I shall denominate it thus, because it belongs to the C column. There will be found an exactly similar WOLF in each of the other three columns. These FOUR WOLVES may very properly be distinguished by the names of the four columns to which they respectively belong; thus we shall have,

"1st, In the C column, THE C WOLF.

"2dly,

"2dly, In the G column, THE G WOLF.

"3dly, In the D column, THE D WOLF.

"4thly, In the A column, THE A WOLF.

"And those FOUR, together with the QUINT WOLF, make the FIVE WOLVES which I have mentioned above. And it was from my having observed these FIVE DISTINCT WOLVES, that I was led to find out that superior mode of tuning keyed instruments which I am now going to describe."

Where does Mr. FAREY find, in what I have said, the FIVE columns which he has thus *inaccurately* mentioned? I speak distinctly of FOUR series of major thirds which are *unalterably distinct* from each other. I represent them in FOUR columns. I expressly mention the THREE *major thirds* which *each* of those FOUR columns contains respectively. I specify FOUR wolves (not FIVE wolves) in the *major thirds*, namely, *one* in each of those FOUR natural and unalterable columns; and I state expressly and most distinctly, that "those FOUR, together with THE QUINT WOLF, make the FIVE WOLVES which I have mentioned above." For, as for the column, which has for its title, at the top of it, these words, viz. "HIGHER OCTAVES *of the keys in the first column*;" it is obvious, from bare inspection, that it is neither more nor less than the FIRST column repeated; and that it contains all the *same* notes precisely, and *none other*; namely, the THREE notes, C, E, and A flat; which are the very notes which compose the C column, or the FIRST of the FOUR series of *major thirds*.

How can this mathematician divide the *twelve series* of major thirds into FIVE equal series? Or, by what means will he divide the number *twelve*, into FIVE equal or aliquot parts? Or how can he contrive to place in FIVE distinct columns, the FOUR natural and unalterable series of THREE major thirds each; without *repeating*, in the fifth column, some one of those four series?

Having explained, in my Treatise, the nature of the C wolf, in the C column; I then say, that "*there will be found an exactly* SIMILAR WOLF *in each of the other three columns*." That is to say, in the G column, in the D column,

lumn, and in the A column respectively. This is accurate language. But, because the FOUR wolves in the major thirds, are SIMILAR to each other; Mr. FAREY seems displeased that I do not consider them as ONE.

A half-guinea, a half-crown, a six-pence, and a halfpenny, bear, *as moieties*, the same proportion and relation, to the guinea, the crown, the shilling, and the penny-piece, respectively. Yet, those FOUR first mentioned coins, although *similar* as halves, are not at all *equal*; though, according to Mr. FAREY's mode of reasoning, they ought all FOUR to be considered as exactly ONE and THE SAME.

In like manner, so little are those FOUR wolves *the same*, that they cannot exist even in the same column. They never can interchange places. Nature has (as it were) forbid them to meet, in the same series; and they cannot be substituted for each other. For, the G wolf, for example, in monochord lengths, is as much smaller than the C wolf, as the G itself is smaller than the C.

In my new Temperament, I moreover divide those FOUR wolves *differently*. For, in the C and G columns, I distribute them in *one* way; as appears by my *musical memorandum table*, in page 22. But in the D column I distribute them in a *second* way; and in the A column I distribute them in a *third* way, which is different from either of the former. And I should like to ask Mr. FAREY *what a strange sort of tuning* it would be, if I were to consider those FOUR distinct wolves as ONE only, and therefore to distribute them *alike!!!*

'A very radical defect,' (says Mr. FAREY,) in the application of the *Stanhope Temperament* to practice, as directed by his Lordship, now presents itself in the mistake made in supposing that *equal temperaments* of two successive *thirds*, or of three successive *fifths*, effected by means of *geometric mean proportionals* interposed between the extremes in each case, and on which all his Lordship's calculations are found to be grounded (and to agree very exactly with my calculations upon the same principles), produce "*equality of the beatings*," by which, says his Lordship, "equal *deviations from perfection*" may be correctly ascertained.'

 Mr.

Mr. Farey does here distinctly admit, that *my* calculations agree exactly with *his*, so far as I use my method of *geometrical mean proportionals*, to determine correctly the *bi-equal* thirds, and likewise the *tri-equal* quints. But he imagines that he has found out a very radical defect (as he calls it) in my other mode of obtaining those same major thirds and quints; viz. by means of the "*equality of the beatings*," between such thirds, when compared to such equal thirds; and also between the equal quints, respectively. Mr. Farey's radical mistake seems to consist in this; namely, that he does not understand the *scientific terms* which I have been obliged to use; and that he does not even know the clear and wide difference which there is between "beats" and "beatings."

Beats, as applicable to this subject, are nearly synonymous to *vibrations*. For each *vibration* produces one beat against the air. So that, when, for example, a proper musical string is shortened, the *vibrations* increase in number *inversely* as the length of the string; and the beats increase, of course, in the very same proportion. If the longer string be, for instance, the note C, that shorter string, which will yield the precise sound of the *perfect octave* C next above, will produce, in any given time, exactly *twice* the number of beats that were produced by the longer string. So that, in this case, we have *two* distinct sets of beats, but there are no beatings whatsoever. For there are never any beatings in any case, when either an octave, a quint, a fourth, or a major third, is *quite perfect*. And it is precisely from the *absence* of the beatings (as they are technically called), that we know that the particular musical interval, then under examination, is *perfect*. In like manner, in a *perfect unison* there are no beatings. But, in this case, as in the former, there are *two* sets of beats; although, from the *isochronism* of the vibrations of the two strings in unison (that is to say, from the equality of the number of their respective vibrations in equal times), those *two* sets of beats appear, to the ear, nearly as if they were but *one*.

But, if a unison, an octave, a quint, a fifth, a fourth, or a major

a major third, be made a very little *imperfect*, then we instantly perceive a *slow* BEATING, which is a *third sound* completely distinct from either of the *two* sets of BEATS above mentioned. The BEATING (as every one, except Mr. FAREY, well knows), *increases in quickness*, as the imperfection increases. And it is only by means of the comparative quickness or slowness of the BEATINGS, that the exact *degree of the imperfection* of any musical interval can be ascertained by the human ear.

The BEATINGS which denote "*equal deviations from perfection*," in those intervals which are exactly similar, possess a distinguishing *equality* in their effect upon the ear, by means of which, and by means of which alone, we are capable of ascertaining, with the requisite precision, *the equality of the imperfections* in such corresponding intervals, when carefully compared with each other merely by the ear, unassisted by any *monochordical* or other mechanical contrivance.

This phænomenon I explain as follows. Let us suppose, as an example, that I wish to compare the *tri-equal quint* D A in the first bass septave, with the *tri-equal quint* D A in the middle septave, of a piano forte. If those quints be exactly what they ought to be, if the octaves be tuned perfect, and if the lower D A be struck alone, there will be heard *three* sounds; viz. the BEAT of the first bass D, and the BEAT of the first bass A, and also a *third sound* which is the BEATING of the *tri-equal quint* D A in the first bass septave. In like manner, if the upper D A mentioned above be struck alone, there will also be heard *three* sounds; viz. the BEAT of the middle D, and the BEAT of the middle A; and likewise a *third sound*, which is the BEATING of the *tri-equal quint* D A in the middle septave. Now, let those two D As be struck together, we shall then have *four* BEATS, and also those *two* BEATINGS above specified, which two BEATINGS will have NO BEATING whatsoever between them. But, let either of the two D As be made either a little *more* or a little *less* perfect than a true *tri-equal quint*; then, the two BEATINGS above specified will produce a very striking *third sound*, which might be denominated "THE

BEATING between the two BEATINGS." And, by means of this *inequality* between the *two* original BEATINGS, the *inequality* of the *unaltered tri-equal quint*, and of the *altered* one, will immediately appear.

A good ear will perceive the *inequality* between the *two* original BEATINGS, in the case of those two *unequal quints* being struck even in succession. And, in like manner, any two imperfect quints, or any two imperfect major thirds, may be respectively compared by striking them in succession, and by carefully observing the *difference* between those BEATINGS which they respectively produce.

These are facts, with which Mr. FAREY appears to be wholly unacquainted; for, in his Observations on the Stanhope Temperament, he, throughout, considers BEATS and BEATINGS as *synonymous* terms; although it is most obvious that they are completely and distinctly *different*.

I think, however, that what I have said above will be quite sufficient to *beat* him out of his error, and also to *beat* into him those important musical truths which both you, Sir, and I, are equally desirous to be able generally to inculcate.

I am, Sir, very sincerely yours,

STANHOPE.

XXI. *Cursory Strictures on Modern Art, and particularly Sculpture, in England, previous to the Establishment of the Royal Academy. By* J. FLAXMAN, *Esq.**

IN order to form a just estimate of the benefit which sculpture has derived from the exertions of the present æra in England, it will be necessary to take a cursory view of this art in Europe previous to the period at which the Royal Academy was established in London; and to observe with a little more accuracy its progress in our own country.

In Rome, the centre from which the arts have emanated for centuries past to the surrounding countries, about 150 years since, the taste of Bernini, the Neapolitan sculptor,

* From *The Artist.*

infected

infected and prevailed over the Florentine and Roman schools. He had studied painting, and seems to have been enamoured with the works of Correggio, who, to avoid the dryness of his master, Andrea Mantegna, gave prodigious flow to the lines of his figures and redundance to his draperies; of which Bernini's statues are only caricatures, totally devoid of the painter's ecstatic grace and sentiment. Before he was twenty years old he completed a marble group, the size of nature, of Apollo and Daphne, at the moment the nymph is changing into a laurel tree: the delicate character of the figures, the sprightly expression, the smooth finish of the material, and the light execution of the foliage, so captivated the public taste, that Michel Angelo was forgotten, the antique statues disregarded, and nothing looked on with delight that was not produced by the new favourite. It is true, Bernini showed respectable talents in the group above mentioned; and had he continued to select and study nature with diligence, he might have been a most valuable artist: but sudden success prevented him—he never improved; the immense works crowded on him made him spurn all example, and consider only how he might send out his models and designs most speedily. The attitudes of his figures are much twisted, the heads turned with a meretricious grace, the countenances simper affectedly or are deformed by low passions, the poor and vulgar limbs and bodies are loaded with draperies of such protruding or flying folds, as equally expose the unskilfulness of the artist and the solidity of the material on which he worked; his groups have an unmeaning connection, and his basso relievos are filled up with buildings in perspective, clouds, water, diminished figures, and attempts to represent such aërial effects as break down the boundaries of painting and sculpture, and confound the two arts. Pope Urban the Eighth was patron of this artist, and so passionately did he admire and promote his works, that, not contented with spending immense sums upon them, he took the antient bronze ornaments from the roof in the portico of the Pantheon, to the amount of 186,000 pounds, for Bernini to cast his bizarre and childish baldequin for St. Peter's, and then published their mutual

mutual shame in a boasting Latin inscription affixed to the building he had robbed so shamefully. Thus the pope and the sculptor carried all before them in their time, and sent out a baleful influence which corrupted public taste upwards of one hundred years afterwards.

Rusconi, Mocho, Bolgio, Quesnoy (commonly called Fiamingo), and the inferior sculptors of the time, adopted the popular taste, which their scholars continued, and its last puny and insipid effects are to be seen in the statues at the Fountain of Trevi, and monument of Benedict the Fourteenth, executed by Bracci and Sybilla, in St. Peter's church, about fifty years since.

Nearly the same taste in the arts of design which prevailed in Italy prevailed also in France, as the latter country was supplied with art, or artists, from the former: thus, when Lewis the Fourteenth invited Bernini to come into France, Bernini answered, "that he had no need of *him*, whilst he had such a sculptor as Puget." Puget's works were somewhat more dry and detailed than Bernini's; Girardon's (his cotemporary) were more heavy; but they were all of the same school. The opinion of Bernini confirmed the monarch, and the same bad taste was cultivated in France with as much zeal as it was fostered in Italy; as we see by the works of Bouchardon, Boucher, &c., who continued it to the same time which extinguished its last feeble efforts in both countries.

Spain, Germany, and the other nations of Europe, receiving their supplies of fine art from the two countries above mentioned, were consequently influenced by the same motives and trammelled in the same taste, which was at this period become so degraded as to be at the point of utter dissolution, had not some controlling circumstances arisen, which assisted in its revival.

The king of Naples had, in part, cleared the ruins of Herculaneum and Pompeii, which exposed to view streets, dwelling-houses, temples, theatres, baths, and public places, nearly in the same state as when they were inhabited 1700 years before: these discoveries brought back to the light of day, as it were by miracle, 700 antient paintings, and a

prodigious

prodigious number of bronze statues and busts of the finest Greek sculpture.

The success of these discoveries, and the interest they excited, stimulated the popes, Roman nobility, and antiquarians, to make excavations wherever there was a probability their labours would be rewarded. These researches fortunately recovered from oblivion innumerable pieces of exquisite sculpture; many of the most precious formed the Clementine museum; many enriched the Borghese, Albani, and other collections; several passed into Germany, Holland, Sweden, Russia, France, and Spain: England was not insensible to the opportunity, and several intelligent and spirited individuals profited by this profusion of antient treasure. Such acquisitions roused attention from all quarters; they were eagerly visited, greedily examined, dissertations and memoirs were written concerning them, and systematic inquiries into their principles published. During all this research and analysis, frequent comparisons were made with the modern works, the remains of the bad taste above mentioned, and which were found so deficient in every excellence that they were universally abandoned to contempt. The interested antiquarian, with sordid cruelty, and to raise the price of his own commodity, whispered that modern talents were unequal to the meanest of these productions, and sometimes he found a senseless purchaser, whose only measure of intelligence was the abundance of his wealth; who would pay dearly enough for any thing that was called antient, to be received into the number of the cognoscenti, and join in the outcry against modern ability.

All this, however, brought in a new and severer mode of study among the artists, with a more diligent attention to nature and the antique, and has enabled some of them to exhibit performances much more on a level with the merit of those works than the insensible can feel, or the interested choose to own.

Having marked these phænomena in the hemisphere of art, we should now turn our thoughts more particularly to England, and see in what manner our own country was affected by their influence. Previous to the Reformation, although

although Italian artists were employed in ornamenting our churches and tombs, yet in the old histories, records, and contracts of public buildings, there are abundant names of English painters and sculptors, who appear to have been considered able masters in their time, perhaps not inferior to their Italian fellow-workmen. But after Henry the Eighth's separation from the church of Rome, Elizabeth, proceeding in the reformation, destroyed the pictures and images in the churches; strictly forbidding any thing of the kind to be admitted in future, under the severest penalties, as being catholic and idolatrous. This entirely prevented the exercise of historical painting, or sculpture, in this country; at the very time that Raffaelle and Michel Angelo had brought those arts into the highest estimation on the continent.—The rebellion, in 1648, completed what the reformation had begun; the fanatics defaced whatever they could, that the former inquisition had spared; they broke painted windows and tombs, carried away the monumental brass, and church-plate, crying, " Cursed he be, that doth the work of the Lord deceitfully!"—Thus the artist, terrified by the threats of the sovereign, the denunciation of death or perpetual imprisonment from the laws, and seared by fanatical anathemas, found that his only hope of safety rested upon quitting for ever a profession, which enclosed him on all sides with the prospect of misery and destruction. From this time, and from these causes, we scarcely hear of any attempt at historical art by an Englishman, until it was again called forth by the benign influence of the present reign.

When the liberal spirit of Charles the First desired to adorn the architecture of Whitehall with the graces of painting, he was obliged to seek the artist in a foreign land; he had no subject equal to the task: Rubens and Vandyck were employed: and when the king's bust was to be done, Vandyck painted three views of his face, a front, a side, and a three-quarter, which were sent to Bernini in Rome, by whom it was executed in marble. If our kings and nobility had continued to inhabit castles, as in the feudal times, painting and sculpture would have been but little wanted; for, if the walls

walls of the building were sufficiently strong to resist battery, or shot, and contained retreats to secure the inhabitants from the enemy, the end of that kind of dwelling was answered: but in the times succeeding Charles the First, the improved state of society and knowledge had induced the great to build commodious villas and palaces, in which the architectural distribution made the sister-arts absolutely necessary to uniformity and completion. Still ingenious foreigners were employed for this purpose, whilst the native was treated with contempt, both at home and abroad, for his inability in those arts which law and religion had forbidden him to practise.

As this suppression of ability was extremely impolitic and dishonourable to the country, let us inquire for a moment on what scriptural authority the prohibition which occasioned it was supported. Painting and sculpture were banished from the churches, that they might not be idolatrously worshipped: and this is just; the divine law orders they shall not be worshipped, but utters no prohibition against the arts themselves: on the contrary, divine precept directed images of cherubim to be made, whose wings should extend over the ark of the covenant, and cherubim to be embroidered on the curtains which surrounded it. This decision in favour of the arts being employed for proper purposes in sacred buildings, is so clear and strong, that it could only be overlooked, or opposed, by infatuated bigotry.

A succession of foreign artists, as has been observed, were employed in almost every work of importance, from the time of Charles the First, until within forty years of the present day. The painters, Vandyck, Lely, Verrio, Kneller and Casali, succeeded to each other; as did also the sculptors, Cibber, Gibbons, Scheemakers, Rysbrack, Bertocini, and Roubiliac. This variety of artists (sculptors are more particularly meant) from different countries, French, Flemings, and Italians, sometimes brought the taste of John Goujon or Puget, sometimes a debased imitation of John of Bologna and the Florentine school, and sometimes the taste of Bernini; but never a pure style and sound principles. After the Reformation, the chief employment of sculpture

was

was in sepulchral monuments, which, during the reigns of James the First and his son Charles, were chiefly executed by Frenchmen or Flemings, scholars of John Goujon, still regulated by the principles their master had acquired from Primaticcio, the pupil of Raffaelle. Some of these works have great merit, particularly the tombs of sir John Norris, and sir Francis Vere, in the same chapel with Roubiliac's monument of lady E. Nightingale in Westminster abbey.

The rebuilding of London, in the reign of Charles the Second, gave some employment to sculpture. Cibber's works are the most conspicuous of that period: his mad figures on the Bethlehem gates have a natural sentiment, but are ill drawn; his bas-relief on the pedestal of London monument is not ill conceived, but stiff and clumsy in the execution; his clothed figures in the Royal Exchange strut like dancing-masters, and have the importance of coxcombs. But with all his faults, what he left is far preferable to the succeeding works. The figures in St. Paul's church, and the conversion of the saint in the pediment, partake strongly of Bernini's affectation; and from that time to the establishment of the Royal Academy we must expect to see every piece of sculpture more or less tinctured with the same bad taste, especially the sepulchral monuments, to which, after the statues and basso-relievos last noticed, we must chiefly look for the progress of sculpture amongst us.

It will be proper here to remark that all the Grecian sculpture was arranged in three classes: the group of figures; the single statue; and alto or basso relievo. The first two classes were suited to all insulated situations, and the latter to fill pannels in walls. These classes not only serve all architectural purposes, but adorn, harmonise, and finish its forms: every attempt to make other combinations between sculpture and architecture will be found unreasonable, and degrading to one as well as the other; but Bernini, whose character and works we have already noticed, seems to have thought that he had the privilege of equally subverting art and nature in his works. I shall mention the following instances, although I am afraid their extreme absurdities will prevent such of those from believing the descriptions as have

not

not seen the things themselves. In the area before the church of Santa Maria sopra Minerva he raised a bronze elephant on a pedestal, and on the elephant's back placed an Egyptian obelisk: the architecture of the east window in St. Peter's church he has loaded with many tons weight of stucco clouds, out of which issue huge rays, intended for light or glory, of the same materials, but long and thick enough for the beams of a house. Extravagances of this kind, and many others that he has committed, have fortunately had little effect upon us, because some have been necessarily connected with catholic churches, and others introduced in fountains, which are only frequent in hot countries: we were, however, the dupes of his school, until native genius gained sufficient judgment and strength to correct its errors, and supply a better style of art. Before the time of Bernini, two kinds of sepulchral monuments prevailed; one from the highest antiquity, which was a sarcophagus, either plain, or covered with basso-relievos, with or without the statue of the deceased on its top. The other kind was introduced by Michel Angelo, in the mausoleum of Julius the Second; and those of the Medici family, in the chapel of St. Lorenzo, at Florence. In these the sarcophagus, as in the former kind, was suited to the niche or architecture against which it was placed, and surmounted or surrounded by statues of the deceased and his moral attributes. Both these practices were rational and proper; the one for plainer, the other for more magnificent tombs. This branch of sculpture was of too much importance to be neglected by Bernini; he stripped it of its antient simple grandeur, leaving it neither group, statue, basso-relievo, sarcophagus, or trophy, but an absurd mixture of all, placed against a dark-coloured marble pyramid, and thus sacrificing all that is valuable in sculpture to what he conceived a picturesque effect. The pyramid is, from its immense size, solid base, diminishing upwards, a building intended to last thousands of years: how ridiculous, then, to raise a little pyramid of slab marble, an inch thick, on a neat pedestal, to be the back ground of sculpture, belonging to none of the antient classes, foisted into architecture, with

which

which it has neither connection or harmony, and in which it appears equally disgusting and deformed! The first monuments he raised of this kind were two in the Chigi chapel in the church of Santa Maria del Popolo, in Rome: this novelty soon found its way into every country in Europe; our Westminster abbey is an unfortunate instance of its prevalence. Rysbrack and Roubiliac spread the popularity of this taste in England; but as the first of these sculptors was a mere workman, too insipid to give pleasure, and too dull to offend greatly, we shall dismiss him without further notice. The other deserves more attention. Roubiliac was an enthusiast in his art, possessed of considerable talents: he copied vulgar nature with zeal, and some of his figures seem alive; but their characters are mean, their expressions grimace, and their forms frequently bad; his draperies are worked with great diligence and labour, from the most disagreeable examples in nature, the folds being either heavy or meagre, frequently without a determined general form, and hung on his figures with little meaning. He grouped two figures together (for he never attempted more) better than most of his contemporaries; but his thoughts are conceits, and his compositions epigrams. This artist went to Italy, in company with Mr. Pond, an English painter: he was absent from home three months, going and returning, stayed three days in Rome, and laughed at the sublime remains of antient sculpture! The other sculptors of this time were ordinary men; their faults were common, and their works have no beauty to rescue them from oblivion.

Thus we have seen the nobler efforts of painting and sculpture driven out of the country by reforming violence and puritanical fury; sculpture reduced to the narrow limits of monument-making, and by these means degraded to a sort of trade; and this department supplied from the corrupt source of Bernini's school, and not unfrequently through the worst mediums. In this state the art continued until the establishment of the Royal Academy settled a course of study, both at home and abroad, which developed the powers of English genius, till then unknown to the natives, and denied by foreigners.

XXII. *Ob-*

XXII. *Observations of the Planet lately discovered by Dr.* Olbers, *which he has since named* Vesta, *reduced to the Mean Times at the Meridian of the Royal Observatory at Greenwich; with their Geocentric Longitudes and Latitudes. Communicated by* T. Firminger, *Esq, Assist. R. O. G.*

	Mean time			App. A. R.				App. Dec. N.			Longitude.				Lat. N.		
	h	m	s	s	°	′	″	°	′	″	s	°	′	″	°	′	″
April 27	9	36	17	5	29	0	33	12	55	20	6	4	19	4	12	13	51
29	9	27	44	5	28	50	16	12	51	52	6	4	8	16	12	14	46
30	9	23	30	5	28	45	52	12	49	51	6	4	3	25	12	14	40
May 2	9	15	8	5	28	38	12	12	44	54	6	3	54	22	12	13	11
7	8	54	43	5	28	26	47	12	38	30	6	3	41	17	12	11	53
11	8	38	54	5	28	25	31	12	11	24	6	3	28	51	11	47	37
18	8	12	21	5	28	40	7	11	33	52	6	3	26	37	11	7	31
19	8	8	40	5	28	43	53	11	27	49	6	3	27	34	11	0	30
20	8	5	1	5	28	47	59	11	21	28	6	3	28	41	10	53	3

The above series were reduced from meridional observations: the following do not claim so much merit in point of accuracy, being deduced from comparisons with stars out of the meridian; they are, however, as *accurate* as such observations can be made.

	Mean time.			App. A. R.				App. Dec. N.			Longitude.				Lat. N.		
	h	m	s	s	°	′	″	°	′	″	s	°	′	″	°	′	″
May 23	9	14	10	5	29	1	33	11	0	44	6	3	32	31	10	28	43
31	10	41	27	5	29	59	49	10	1	5	6	4	1	14	9	11	0
June 4	10	56	3	6	0	36	45	9	28	0	5	26	45	40	8	55	10
9	10	7	11	6	1	30	9	8	43	5	5	27	52	48	8	35	23
14	10	24	23	6	2	31	40	7	55	48	5	29	8	22	8	16	35
22	10	29	48	6	4	23	30	6	36	3	6	1	23	18	7	47	0

The above series will be found extremely useful to astronomers, who may be enabled *from them* to carry on their observations of this planet; and they will also greatly assist the calculator in computing its orbit.

XXIII. *On the Decomposition of Light into its most simple Elements, being Part of a Work upon Colours.* By C. A. Prieur, *late a Colonel in the Corps of Engineers**.

The complex state of white light, which is that kind with which we are most habitually acquainted, was discovered by Newton. Since that period nothing important has been added to the discoveries of that great philosopher; and the objections made by some to this part of his experiments, proceed either from want of examination, or from new experiments ill understood. They establish irresistibly the various refrangibility of the particles of white light; the different colours of these particles; and lastly, that the colours of bodies are alone owing to the property they possess of throwing out, reflecting, or transmitting some of these particles or rays more than others.

Hence it follows, that, independently of the refractive power, which is proper, in fact, for analysing light, every coloured body presents by its action another method of making this analysis more or less completely.

Before discussing this last method, which is my present object in particular, it may be useful to examine the first method, with the view of thereby clearing up or confirming several peculiarities.

Although the question has been often agitated, it has never yet been satisfactorily resolved, viz. if there are really seven distinct classes of colours in the solar spectrum, or rather a single series of shades from the commencement to the end, blended in an insensible manner. Some philosophers are even uncertain if Newton has distinctly affirmed one or other of the propositions.

On the first view, the opinion of this father of optics does not appear doubtful; for he relates that a person, whose sight was better than his own, was requested by himself to draw across the spectrum lines on the confines of each colour; and that the results of this operation, several times repeated, agreeing tolerably well, he then fixed definitively

* From *Annales de Chimie*, tom. lix. p. 227.

the

the spaces of each colour, and found that they had a certain relation with the tones of a musical gamut *. This same division, expressed in cyphers, serves Newton afterwards for establishing different calculations, and particularly the method by which he determined the course of the shades of coloured rings: he returns, besides, in several places of his work, to speak of the confines of this and that colour, and directs his experiments accordingly. We cannot suppose, therefore, that he was not persuaded himself of this fundamental proposition, or that he hesitated to announce it positively.

With respect to the truth of the proposition itself, it is easy to ascertain it in several ways.

1st, By repeating Newton's experiment, which is the seventh of the second book of the first part of his Optics, it is merely sufficient, without searching for the utmost exactitude, to procure in a dark chamber a solar spectrum not too confined in its breadth. We there see distinctly enough, if not all the sections of the colours, at least some of them, particularly in the region of the most refrangible rays. If we also arrange an apparatus for giving a very long and very narrow spectrum by means of a small lens placed before the prism; then, by receiving the image upon a card rather nearer the lens than its focus, we shall there find the blue in particular, terminated at each end by a straight line, very clearly defined.

2d, The disk of the moon, when full, and far from the horizon, observed with a simple prism, displays upon her lengthened image several circles which indicate separations of colours perfectly unblended.

3d, Examine also with the prism the coloured fringes of a white rectangular body, a little large, and well illuminated, you will remark on one side a red stripe falling into the yellow abruptly, which, in its turn, loses itself in the white; and, on the other hand, some blue, distinctly enough between the white and a violet stripe: from this it would follow that there are only four kinds of colours.

* Optics, book i. part 2. problem 1.

 4th, Lastly,

4th, Lastly, if we look at a white and narrow body placed upon a black ground, such as a strip of paper, a thread of silk, or a metallic needle, there will only appear, for one of these objects, if we are properly placed, three colours, red, green, and violet; and scarcely can we perceive either in the one or the other any softening of the shades.

These various phænomena are incompatible with the supposition of an uniform gradation of colours throughout the whole length of the spectrum, that is to say, incompatible with a variation of strength, owing to one sole and uniform law; but they do not contradict the results of the experiment which gave to Newton seven classes of particular colours. If, in some cases, as we shall soon see, refrangibility reduces the apparent colours to four principal kinds, or to three, or at other times shows them in a state more or less confused, it is an irresistible consequence of the relation existing between the breadth of the body observed, the angle of dispersion of the rays, and the distance at which the image is received.

We may, by means of a figure, account for these effects: it results from this, that, in order to obtain by refraction the most colours when the rays are primitively parallel, there must be a very small luminous object, a considerable angle of dispersion, and a very great removal of the image: hence the difficulty of repeating with success the experiment of Newton, where he proposes to disengage as much as possible the heterogeneous rays from each other *.

This we may also infer from the very interesting experiments of M. Rochon upon the light of the fixed stars †. This philosopher undertook these examinations on account of the advantage of the smallness of the apparent diameter of these stars; and in order to attain his object, he ingeniously arranged before an excellent telescope an achromatic prism, of his own invention, with a variable angle: with this apparatus, and the refracting angle of his prism being 14° only, he saw among others the star Sirius, the spectrum of which was almost entirely composed of three co-

* Optics, book i. part 1. exper. xi.

† Recueil de Memoires, p. 13.

lours;

lours; viz. red, green, and violet; the two former yielding between them a feeble shade of yellow, and the two last also a slight gradation. It is evident from what precedes, that here there must have been wanting some condition requisite for the greater simplification of the light; this condition is correspondent with a greater removal of the image, or a more considerable angle of dispersion. The author soon afterwards increased the refracting angle to 50°, upon which depends that of dispersion; but another cause then comes to alter the results. In proportion to the increase of the thickness of the prism, several particles of the light became too feeble to be transmitted; the colours disappeared gradually in a certain order, and at last nothing remained except one of a straw-coloured yellow. The comparison of the spectra of stars of different magnitudes affords the means of observing the same gradation in the penetrating power of the various luminous rays; a power which is nothing else than their illuminating faculty pointed out by Newton in this same order.

These experiments, therefore, do not alter in any respect the data admitted as to the species of the component particles of light; on the contrary, they confirm them.

But some discoveries posterior to those of Newton, made by effecting the refraction by different bodies, have taught us that the particular distribution of rays upon the spectrum was not invariable. According to the nature of these bodies, and independently of their density and of the aperture of the refracting angle, we sometimes see the angle of mean refraction changed, as well as that of the dispersion of the extreme rays, and sometimes also the relative separation of the intermediate colours, but without their order being inverted; at least, there is no simultaneous action of several refracting substances properly combined. Thence is derived, as we know, the principle of the achromatic glasses so admirably constructed, first by Dollond, and since perfected (as he thought) by Dr. Blair *. To this last author we are in-

* Edinburgh Philosophical Transactions, vol. iii.

debted for a very interesting collection of experiments upon this subject.

In some of these he determines, with a great deal of care, the dispersive power of various substances. He finds some of them which possess it in a degree considerably stronger than that of common or *crown glass*; such as, for example, sal-ammoniac, the muriatic acid in particular, and, above all, the smoking muriate of antimony. But what is not less remarkable, is, the displacing of the limits of the colours in the dispersions caused by these different substances; for the less dispersive substances, in general, cause the mean direction to pass by the junction of the green and the blue; while other substances, such as *flint glass*, essential oils, and metallic solutions, throw the limit of these same colours much nearer the red; and it is, on the contrary, carried again towards the violet upon the spectrum produced by the muriatic acid. These varieties induced Dr. Blair to prepare object glasses more truly achromatic than the common ones. He made them of several forms, one, among the rest, by means of an assemblage of curved glasses, between which he enclosed a solution of muriate of antimony, mixed with muriatic acid, to the precise quantity necessary for compensating by its action, for the displacing of the green rays, which the other substance would have occasioned.

In short, we may form an idea of the comparative exactitude of the methods of Dollond and Blair, by means of two very simple figures, on which we trace the course of the rays, with reference to the axis of each object glass. There will result, 1st, That object glasses made by these two processes are both susceptible of giving a circular focus of white light exempt from iris, but a little broader in Dollond's: 2d, Dr. Blair's does not necessarily unite all the kinds of rays, as he thinks it does: this does not lessen, however, the practical advantages of the instrument, for the perfection of which we are indebted to him.

I have dwelt some time upon these details, because they appeared to me to be connected with my subject: I shall now

now give a recapitulation of all that precedes, which I shall endeavour to do with precision.

White light is decomposed by refraction into an infinite number of parts or rays. They have a different colour at every point in the length of the spectrum; and this colour cannot be varied by a new refraction, if the simplification of the spectrum be at the degree to which Newton carried it. Though in this simplified spectrum the lines of demarcation between the colours are by no means very perceptible, it is impossible, however, to ascribe the gradations of their tints to one and the same law. Various observations establish the existence of several distinct species of colours; and their division into seven classes, as given by Newton, agrees with a great number of phænomena. Yet some bodies, by their peculiar refractive power, derange the spaces of the colours in the spectrum; for example, the green rays are, in some instances, brought nearer the red, in others nearer the violet; which proves that the dispersion of the rays does not depend absolutely on their own nature. Such are the principal observations it appeared to me necessary to make, in order to show the present state of our knowledge; and I shall now proceed to examine the action of coloured bodies upon light.

I have shown * that all kinds of transparent bodies, of different colours, which I have observed, transmit ultimately only the red, or green, or violet rays. The progressive absorption never finishes by any other colours, and I long sought in vain for a substance in which the final absorption should be of the yellow or blue rays. Such a result could not fail to excite my attention. I remarked, that, under certain circumstances, the colours exhibited by refraction were almost wholly these three, red, green, and violet; that sometimes yellow appeared to arise from a mixture of red and green, and blue from a mixture of green and violet; which my dial †, as well as the placing of certain coloured glasses

* In my Memoir read at the Institute on the 11th of August 1806.

† This dial is simply a circle exhibiting the seven primitive colours conformably

glasses on each other, indicated as possible. I perceived too, that the tints of the seven orders of colours might be imitated by the three primitive colours alone which I have mentioned*. This was sufficient to suggest to me the idea, that perhaps these three kinds of rays were all that really existed; a proposition that required to be examined with care proportioned to its importance. Accordingly I inquired into the probabilities that might be brought to support it, and compared it with all the phænomena of colour that occurred to me; and lastly I proved it by direct experiments. The details of these I shall reserve for the last place, beginning with an account of the others.

I have already stated that the supposition of three colours was not inconsistent with the formation of all the tints of the spectrum. Nor does it contradict the unchangeableness of each tint by a second refraction; for if a red ray of a certain degree exists on the spectrum at the same place with a green ray of a particular degree, their combination will give a yellow of a particular tint; and as these two rays have the same refrangibility, a similar refractive power cannot again separate them. Accordingly, to have a spectrum in all points similar to that which really occurs, nothing more is necessary than to conceive it composed of three spectrums partly overlaying each other; one formed of red rays, differently refrangible, and of different tints; another trenching a little upon the first, and having only green rays, but a similar gradation of tints corresponding to their refrangibility; and a third, exhibiting an analogous series of violet rays, and in like manner trenching upon the green. On this hypothesis, there will be no disruption of the whole image, whatever extent be given to it by refraction: besides, it accounts for

formably to the ideas of Newton. See Optics, book i. prob. 2. The author has explained the principles of the construction of this dial, and its leading properties, in his work.

* This proposition of three primitive colours is very different from that formerly adopted; for the red, yellow and blue, have hitherto been so considered; while here they are the red, green, and violet, the exclusive existence of which is proved by the analysis of white light in several ways, as will be seen further on.

seven

seven colours separated by lines of demarcation, which no one has yet explained.

To comprehend this, let us look at fig. 1. Pl. IV. which is constructed in the following manner: A right line is divided into seven parts, proportioned to the spaces of the seven colours in the spectrum, and marked by the initials of those colours. On each of the points of division I have erected an ordinate, and afterward drawn the arbitrary inclined line *a d*, then *b g* cutting the former in *c*, and lastly *e h* cutting the preceding in *f*. I suppose that the modifications of the red rays, on which their different refrangibility depends, are represented by the ordinates corresponding to the line *a d*: these quantities express nothing relative either to the velocity of the rays, or the magnitude of their particles; perhaps they may have a relation to their density, or to any other quality whatever that constitutes their difference. In Newton's system of seven classes of primitive colours, there are likewise red rays differently refrangible; this therefore is not a difficulty peculiar to the state of things I am examining. In like manner the ordinates of the line *g b* will be the modifications of the green; and those of the line *h e* the modifications of the violet. This being understood, it becomes evident that the first division of colours from *a* to *b* will be red alone; that it will be followed by a mixture or combination of green and red from *b* to *c*, in which the quantity of the latter will predominate more and more, and consequently give orange; after which another mixture of red and green will proceed from *c* to *d*, in which the green will predominate more and more, forming yellow; then from *d* to *e* will be green alone; from *e* to *f* the mixture of green and violet that produces blue; from *f* to *g* the mixture producing indigo; and lastly from *g* to *h* pure violet.

But another very striking property of the spectrum, which has not hitherto been explained, is the greater brightness of the yellow compared with all the others. This proceeds evidently in my figure from being the sum of the red and of the green in the same space. In the blue too there is

is an augmentation of light by the union of the green and violet; but the effect is much less than in the preceding instance, both from the nature of those colours, and their extent, though there is some trace of it in the spectrum when properly displayed. In short, I have not by this figure pretended to exhibit any thing more than what may possibly happen. For this reason I have limited the ordinates of each colour by a right line merely; for, as the law of their progression is not known, so that it is impossible to give the precise curve, I have adopted the simplest line as sufficient for my purpose.

The striking agreement of my hypothesis with the peculiarities of the spectrum excited me the more to apply it to the dial of colours. This coloured figure has such singular properties, that the mind cannot easily bend itself to them. How indeed can it conceive the existence of an infinite number of luminous rays, all different yet equally simple? How is it, that taken in pairs from the extremities of every diameter of the dial, that is, from any two opposite points, they shall always form the same white? For instance, a certain red ray with a green gives white; an orange with a blue, the same; a violet with a yellow, still the same. What a strange similitude! How again are the seven distinct orders of the spectrum consistent with that insensible blending of the tints of the dial recommended by Newton, and in fact necessary? Yet all these are so completely supported by experiment, that their reality cannot be questioned.

I had therefore a problem to solve, the complicated data of which seemed at first not to promise a simple solution; yet, after various attempts, I attained my object, as will be seen.

[To be continued.]

XXIV. *On*

XXIV. *On the Alkalies of Commerce, and on the least expensive Process for ascertaining their commercial Value by Means of the Instrument called the Alkali-meter*. Read to the Academy of Rouen, 5 Thermidore, An. 13, by* M. Descroizilles *senior*†.

American Potash; Process for obtaining it.

The name of American potash is given to an alkaline substance, in compact masses which have visibly undergone fusion. This alkali seems to be the produce of a ley of ashes and lime, with the addition of sea-salt. The result is a very deliquescent and caustic substance, susceptible of entering into fusion long before becoming red-hot. If the proportions of sea-salt and lime have been sufficient, all the potash is combined with the acid of the former, and has separated the soda, at the same time that the lime is precipitated in carbonate: thus this kind of alkali must be considered as a mixture of caustic soda and muriate of potash. That which is red owes its colour to sulphur and charcoal: it also partly owes this colour to a little iron. There are first, second and third sorts of American potash. The laundresses of Paris frequently pay much more for them than they are worth in point of alkaline properties. The same effects might be produced if they added to common potash or to wood ashes a small quanty of quick-lime, with this difference, however, that their expense would be considerably diminished. To conclude, all the American potashes I examined also contain carbonic acid. In the departments of France where salt is made, the mixture of sea-salt and lime with ashes, before their lixiviation, might be turned to great profit. It is a very simple operation, and I only speak of it from some experiments made upon a very large scale,

* N.B. The first part of this memoir consisting merely of some well known facts relative to the origin and various forms and names of the alkalies of commerce, we give here only the new points of view in which potash and soda are presented.—*Note by the French Editor.*

† From *Annales de Chimie,* tom. lx. p. 17.

which

which have convinced me of the benefits that might be obtained, particularly during a naval war, which sometimes doubles the price of the American potash.

Advantages resulting from the Substitution of Salt of Soda in Place of native Soda.

Among the best processes known for obtaining the salt or carbonate of soda, some of them are susceptible of improvements, which have not yet been published. A chemist, who is unaccustomed to operations on a large scale, will be able to judge which of these processes deserves the preference in point of facility or œconomy.

M. Pelletan jun. has presented to the academy of Rouen some salt of soda of his own manufacture. The experiments of the committee of which I was a member, and those of some dyers of scarlet cotton, prove that this salt of soda of M. Pelletan has given results which, in point of beauty, may be compared with those of the best Spanish soda, and that the employment of this salt was much more œconomical.

As to its œconomy, we find that this last point depends upon the relative prices of soda and of salt of soda. In all cases, it will depend upon the manufacturers of this salt to preserve to it the particular advantage of a constant degree of alkalinity, while the sodas of commerce often vary among themselves as 1 to 2: the employment of salt of soda, therefore, requires much less workmanship and time.

Prospect of a speedy Manufacture of this Salt of Soda in France upon a very large Scale.

Several other manufacturing chemists have proposed the manufacture of salt of soda, the demand for which, if it could supply the place of the soda of Spain in all its applications, might become so extensive, that foreign sodas would fall into disrepute.

I advised M. Arnaud to send into France the salt of the Spanish sodas deprived of water by desiccation, which would in some measure diminish the expense of carriage

and

and custom-house duties. I gave this advice to M. Pelletan also. This salt does not always crystallize with facility, particularly in warm seasons: by this method the manufacture will go on much faster *.

Necessity of a prompt and easy Method of proving the Alkalies.

Among the various alkalies of commerce, none of them offer either soda or potash in their state of purity. The least impure are those which contain merely carbonic acid and water: afterwards come those which contain earths, carbonate of lime, sulphur, charcoal, and particularly the alkaline muriates, and sulphates, of which it is but too true that for some years a fraudulent mixture has been made, extremely prejudicial to manufactures in which alkali is used.

For a long time it has been a great desideratum to discover a prompt and easy process for trying potash, soda, natron, and the other alkalies, which might be both within the reach of buyers, and certain in its results. The areometer, so convenient in the spirit-trade, and even in that of the sulphuric, nitric, and muriatic acids, is insufficient in this respect. It has been successively suggested to employ

* Since writing the above memoir, I learnt that M. Carny, to whom France is indebted for a remarkably expeditious process of manufacturing gunpowder, has formed an establishment at Dieuze, where he has already, within one year, manufactured twelve hundred quintals of an excellent salt of soda, which he sells under three different forms. The first is in a very dry powder. The second is in irregular masses, and still contains some water of crystallization. The third is in well formed and colourless crystals. No. 1 equals in alkaline strength the best potash extracted from tartar. M. Carny sells it at 200 livres for 100 kilogrammes delivered at Rouen, while the fine Russian potash is sold at the same price, although 10 per cent, weaker. I rejoice in having an opportunity of publishing this new obligation of our country towards one of its most respectable citizens, who has not as yet received a recompense proportioned to his merit. The experiments begun at Rouen, under the direction of M. Vitalis, upon dyeing cotton red after the Adrianople method, promise the most happy results; they already produce a saving of 70 per cent. from the employment of this salt, compared with the best Spanish soda. The manufacture of M. Carny might be raised to 2,500 metrical quintals annually; it is founded upon the œconomisation of the sulphate of soda, of which the salt springs in France present us with large quantities.

various

various acids, and some of their combinations with precipitable bases. The sulphuric acid pure and uncombined, in my opinion, deserves the preference, as it is found every where at a low price, and can be brought to the same state of concentration.

The process which I found to be the most expeditious for judging comparatively of the value of various specimens of alkali, consists in ascertaining how many centiemes of their weight they require of sulphuric acid for their saturation. If this process is not as yet within the reach of all buyers, we shall easily find at least persons intelligent enough to execute it well; for, independently of other chemists, there are apothecaries everywhere who can easily make such trials.

Description of the Alkali-meter.

The instrument which I have called the *Alkali-meter*, is a glass tube eight or nine inches long, and seven or eight lines in diameter. It is closed at one end, the other terminated in a kind of small funnel, with a beak *a* (fig. 3, Plate IV.) adhering to the tube by a neck with an aperture of two lines and a half. Upon the shoulder which supports this neck is a hole *b*, for letting the air out and in. Fig. 3. is a vertical section of this instrument mounted upon a stand *c*, *d*, in which it is solidly fixed with mastic, by means of a kind of hook (shown at *c*, *d*) left for this purpose at the lower extremity of the tube. In order to facilitate the carriage of this instrument, it is furnished with a kind of tin case, without a bottom, having a lid *e*: the other part *f*, being, like the former, indicated by punctured lines, is a tube open at both ends, and upon which, in order to fasten the lid, there must be a swelling *g*, *g*.

The alkali-meter ought to contain easily 38 grammes, or 76 demi-grammes, because each division, or degree, which we trace afterwards upon the instrument, represents a demi-gramme of this liquor, which it is extremely important to proportion accurately as follows:

Alkali-metric Liquor.

Take concentrated sulphuric acid, or oil of vitriol of commerce,

merce, at 66 degrees of Baumé's areometer; afterwards put in equilibrium, in a good balance, a vessel of porcelain or glass, and pour into it a given weight of the above acid, say a hectogramme; add carefully, on account of the caloric which is liberated, nine hectogrammes of pure water, then stir it well with a glass rod; and then put it into a bottle, which you must cork up, that no alteration, either by dust or by evaporation, may take place.

Graduation of the Alkali-meter.

The alkali-meter is then graduated by given weights of this liquor in the following manner: Place the instrument in perfect equilibrium in a balance, and introduce into it very exactly two grammes or four demi-grammes of the test liquor; place the tube afterwards in a vertical position, and mark the level by a small scratch with a diamond; pour in once more, and all at one time, 36 grammes or 62 demi-grammes of the liquor, and mark this level also by a scratch: after this, empty the instrument, and draw a mark on the other four vertical parallel lines; and forming three spaces of about one line between each, mark on the two extremities two small transverse lines, making right angles with the other four; then write 0 in the upper part, and 72 in the lower; pour liquor into the tube afterwards, up to the point marked 72; put the instrument once more in equilibrium, and introduce one after the other 71 demi-grammes of the test liquor, taking care to mark, each time, one point in the middle scale: this being accomplished, trace regularly, by four and four points, lines across the three scales; then trace in the same manner, upon all the other points, lines through the middle scale only: engrave, after this, on one side, and opposite to each fourth division, the cyphers 4. 8. 12. 16. 20. 24. 28. 32. 36. 40. 44. 48. 52. 56. 60. 64. 68. and 72: the engraved figure of the alkali-meter will render this description more intelligible.

I shall, by and by, inform my readers how and wherefore I arranged this instrument so as, if necessary, to supply the place of the Bertholli-meter. The marks are made with very little trouble by employing a glazier's diamond.

Alkali-

Alkali-metrical Requisites.

Besides the alkali-meter and the test liquor the following articles must be procured:

1. Syrup of violets.

2. A small balance: those for weighing gold coins will answer; we may employ smaller, however.

3. Weights of 2 drachms 44 grains and two-thirds of a grain.

4. A measure of a demi-decilitre, or rather, according to the old system, the small measure answering to the sixteenth of a Paris pint (those which have no lids are preferable); this small vessel is intended to give any two measures of water, and its capacity is not of very great consequence: any other vessel of nearly the same capacity will answer.

5. Common drinking-glasses: those whose edges are turned outwards should be preferred: if common glasses only are used, before decanting a portion of the liquor into them as I have described, we must give a slight coating of oil, fat or grease, to part of their orifice, which will present an irregular flowing down the outer sides.

6. Small chips of wood or matches from which the sulphured ends have been taken off.

7. A bottle of water and an earthen plate.

8. Lastly, for soda, hard American potash, and natron, there must be a metal pestle and mortar of about six inches in its greatest diameter.

The whole being arranged, you may easily proceed to the trial of any given alkali; we shall take potash as an example.

Alkali-metrical Trials of Potash.

Weigh exactly one decagramme, or two drachms $44\frac{2}{3}$ grains of potash; put it into a glass, and pour upon it about four-fifths of a demi-decilitre of water; facilitate the solution of the potash by stirring it with a small chip of wood three or four times in an hour and a half, a minute each at each time. When you think the solution is effected, pour it

it into the small tin measure, which you then fill up with water; pour it back again into the glass, in which you must still pour a measure full of pure water: stir this new mixture also three or four times within half an hour, in order to facilitate the precipitation of a slight sediment, which soon falls down. This sediment being completely formed, slope the glass with caution, in order to fill with clear liquor the small measure; then empty this last into another large glass; after this place round the edges of a plate drops of syrup of violets; pour also into the alkali-meter test liquor until the line marks 0: take it afterwards with the left hand, inclining it upon the glass which contains the moiety of the clean alkaline solution: the acid liquor will fall into it by hasty drops, or in a very small thread, which you may moderate at pleasure by retarding the entrance of the air at the lateral hole or vent, upon which must be placed the end of the finger; at the same time, with a small stick or match assist the mixture, and facilitate the development of the carbonic acid, which is manifested by effervescence. When you have emptied the alkali-meter to about the line marked 40, try if the saturation approaches, by drawing your small stick from the mixture and resting it upon one of the drops of syrup of violets, which should become green if the potash is not of a very inferior quality. If, on the contrary, the violet colour is not altered, or, what would be worse, if it be changed into red, there would be in the first case an indication of saturation, and in the second a proof of supersaturation. But this is not the case with good potashes: at that line, the liquor tried can alter the syrup of violets into green only; or cause to return to the violet, and even to the green, the drops which had been changed into red at the time of a former trial: we must, therefore, in general add more acid, which occasions a new effervescence; this addition must be always made with caution, and we must touch every time a drop of syrup of violets in order to stop. When at last the latter assumes a red hue, then, after having restored the alkali-meter to a perpendicular position, in order to see at what line the testing liquor stops, you must reckon

 one

one degree less in order to compensate the excess of saturation. The mean term of potashes is 55: this implies that they require for their saturation fifty-five hundredths of their weight of sulphuric acid.

[To be continued.]

XXV. *Notices respecting new Books.*

The Code of Health and Longevity; or, A concise View of the Principles calculated for the Preservation of Health, and the Attainment of long Life. Being an Attempt to prove the Practicability of condensing, within a narrow Compass, the most material Information hitherto accumulated, regarding the different Arts and Sciences, or any particular Branch thereof. By Sir JOHN SINCLAIR, *Bart.*—4. Vols. 8vo.

SIR JOHN SINCLAIR observes, that the multitude of books published on the subject of health are so exceedingly numerous, that it almost requires a life to read through the whole, and that the only satisfaction that can arise is from this aggregated knowledge, collected from certain parts contained in each work. With astonishing industry the indefatigable baronet has waded through the stores of antient and modern lore, and culled from each what appeared to him adapted to the purpose. His Code of Health and Longevity is therefore not an original production, founded upon his own experience; but extracts from all the learned writers on that subject in every age and country of the world. This plan first suggested itself to Dr. Thornton, which he brought to some degree of perfection in the Philosophy of Medicine, or, as he has ingenuously confessed in the title-page, Medical Extracts on the Nature of Health and Disease; and sir John Sinclair has made the greatest use of this able performance, everywhere quoting it as a work of the first authority; and indeed the Code of Health is an excellent companion to the Philosophy of Medicine, filling

up

up in detail what that learned and ingenious physician has in several places only slightly touched upon, fearful perhaps of making his work too voluminous, and reserving himself more particularly to what related to disease, where doctor Thornton is sufficiently in detail and full, and which seemed most to demand the attention of a physician. The Code of Health opens with preliminary observations "On the advantages to be derived from arranging and condensing the knowledge already accumulated." The author here judiciously observes, "That, in the present state of literature, knowledge may be compared to a small portion of gold, dispersed throughout a great quantity of ore. In its rude condition, the strongest man cannot bear its weight, or convey it to a distance; but when the pure metal is separated from the dross, even a child may carry it without difficulty."

The baronet then states what particularly led him to form on this plan a work on Health. The following apology is given:—

"Though naturally possessed of a sound constitution, untainted by any hereditary disease, yet, about six or seven years ago, the author had fallen into a weak and enervated state, and found himself unequal to the task of managing his own private concerns, of prosecuting useful inquiries, or of applying his mind to political pursuits, with his former energy and zeal.

"As age advanced, he found many of his contemporaries either getting into a declined state, or sinking into the grave, much sooner than he had expected. The causes of these events he naturally wished to ascertain, and to determine whether so premature a decay might not have been prevented.

"In carrying on his Statistical Inquiries, (a branch of which, namely, the effects of climate, was necessarily connected with the points in question,) it was a matter of astonishment to him to find, how few of the human species, in proportion to the numbers born, attained any considerable extent of years, even in the healthiest countries.

"Above all, it was a matter of regret (and the longer one

lives the more striking it becomes), that even while men do live, their existence is too often embittered by disease, and their life a burden both to themselves and to others.

"These circumstances united, naturally directed his attention to the subjects of Health and Longevity.

"He began by endeavouring to procure the re-establishment of his own health, in which, with the assistance of some eminent physicians, he has happily succeeded; in so much that he is now enabled to take as much mental and bodily labour, as any person, it may be presumed, could undergo at this time, who was born in the year 1754, who had fallen into a weak and delicate state, and who had, for above thirty years past, been engaged in laborious and exhausting pursuits. Indeed, nothing but a complete restoration of health could have induced him to engage in a work like the one which he has now undertaken, and which must necessarily be attended with no common degree of fatigue and exertion.

"He next ventured to give hints to others, whether advanced in life, or in a sickly state, how they might secure the same advantages which he had derived from his extensive inquiries; and he has had the satisfaction of receiving the most grateful acknowledgments from various persons, in all ranks of life, for the benefits they have received, by the adoption of the rules which he recommended.

"Thus, confirmed in the opinion he had formed of the advantages to be obtained from such inquiries, he was at last induced to think of a greater and bolder attempt, '*That of instructing his fellow-creatures in general, how they could best preserve their health, and attain a comfortable old age.*'

"If an antient maxim be true, *Experientia docet*, the author has some pretensions to a knowledge of these subjects, from his own observation and experience; and, in the progress of the intended publication, he will necessarily detail the means by which his own health was restored. It will thence appear, that the preservation and the restoration of health depend much on minute and unremitting attentions to clothing, diet, air, exercise, habits, &c. which, taken singly, appear even trifling, but when combined, and regularly

larly observed, are of infinite importance. Rules, with respect to these particulars, he was at considerable pains to collect from a number of intelligent individuals now living, or lately deceased, as well as from personal observation. He considers them to be a valuable part of the intended publication, as several of them are not to be met with in any work hitherto printed. Had they been known to him thirty years ago, his own health would not have suffered as it did, and many irksome hours would have been avoided. By the observance of these rules, he is persuaded that men, instead of living recluse, or weighing their food, measuring their drink, and the like niceties, may, with moderation and prudence for their guides, mingle in the usual train of civilized or artificial society, without suffering in their health, or shortening the period of their existence."

Chapter I. details the Requisites for Long Life, derived chiefly from nature, a very interesting chapter. Chapter II. relates to the Mind, and the Regulation of the Passions. Chapter III. is on the Climate and Place where a Person resides. Chapter IV. On the Nature of Air, and of its Importance to Health. Chapter V. Of liquid Food. Chapter VI. Of solid Food. Chapter VII. On Digestion and the Effects thereof. Chapter VIII. On Exercise. Chapter IX. On Sleep.

It is but justice to Dr. Thornton to state, that in his Philosophy of Medicine the very same titles appear; and our first obligations are therefore to that physician. The plan also of this kind of performance was derived from him, and the merit of such a work rests entirely on the judgment of the selection, and combination of the knowledge, so as to form a satisfactory performance.

The public are much indebted to Dr. Thornton, who first chalked out this perfection of literature, which has been imitated by two such men as sir John Sinclair in his Code of Health, and Dr. Darwin in his Phytologia. It is but justice to say, that each have their respective merits; and, as Dr. Thornton's Extracts are designed chiefly for students, that he has been less elaborate in detail; and what renders his Medical and Botanical Extracts of most value is, the many

 accurate,

accurate, useful, and superb plates, whereby science has been enriched and embellished.

The three remaining volumes are the entire works of some of the most eminent writers on the preservation of health and the prolongation of life, with the correspondences on this subject of several of the first medical characters in Europe: this part therefore contains much new and original information to the public, especially in what relates to exercise, where the training of boxers, horses, &c. is amply detailed; so that we must recommend this work to all classes of persons. The physician already conversant on the subject from his own library, will not regret to see similar publications in a similar dress, and assembled. In the present work he will also find much new information for his meditation and improvement.

Théorie générale de l'Electricité, du Galvanisme, et du Magnetisme. Par H. AZAIS, *Author of Essai sur le Monde, Mémoire sur le Mouvement moleculaire et sur la Chaleur, &c. &c.*

M. Azais is, we understand, a candidate for the prize, offered by the emperor Napoleon, for the best memoir on the above subject. His work is preparing for the press at Paris; in the mean time he has published the following syllabus of its contents:

" When light and caloric, in a sufficient quantity, are united to the elements of water, they make them enter into a state of gaseous expansion, and produce with them the two gases, oxygen and hydrogen.

" If afterwards the progress of expansion continues, the two gases are separated and attenuated; and when each gaseous molecule is reduced to the utmost tenuity of which it is susceptible, it becomes the *molecule of the electrical fluid.*

" Thus there are two electrical fluids; the *oxygenated* and the *hydrogenated fluids.* These two fluids are formed, combined, and renewed incessantly; they traverse perpetually, and in an inverse ratio to each other, the envelope of the earth—its atmosphere. They proceed continually from the

the equator towards the poles, and from the poles towards the equator. By their movements and combinations they produce all the phænomena of *electricity*, *galvanism*, and *magnetism*.

"When they are rapidly caught by friction machines, as they are called; and when the movement given to them does not allow them time enough to separate from each other, they show themselves in the state of mixture; they produce the phænomena of ordinary or mixed *electricity*.

"When they are caught by the Galvanic apparatus they are separated from each other; each is projected towards one of the two extremities of the apparatus. They then produce all the phænomena of *galvanism*. They produce, among other phænomena, their own reduction into perceptible gases when we receive them in a vessel full of water, where, by the effect of the pressure of the liquid, they are constrained to condense: the oxygenated fluid resumes the state of oxygen gas; the hydrogenated fluid resumes that of hydrogen gas. As in friction machines the two fluids are always confounded, the condensation of these two fluids by the pressure of water produces a mixture of oxygen and hydrogen gases.

"All living beings are electrical apparatuses, in the interior of which sometimes the phænomena of mixed electricity take place, sometimes the phænomena of doubled electricity, or galvanism.

"When the two electrical fluids, abandoned to their natural course, meet with bodies which have an equal affinity for one and the other, they give to these bodies, by penetrating incessantly into them, the *magnetical constitution*. It is in this manner they produce, by the help of these bodies, all the phænomena of magnetism.

"The oxygenated fluid is the principle of acid compositions; the hydrogenated fluid is the principle of alkaline compositions. These two fluids, by penetrating and pressing continually into the heart of the sea, are partly combined with aqueous molecules; and thus form on the one hand muriatic acid, and on the other hand soda. These two compositions are also combined by the effect of the

 continual

continual movement of the waters. This is the principal source of the muriate of soda, with which the water of the sea is charged. When the sea is very rough, the friction of the two electrical fluids against each other liberates a part of their light: this is therefore what makes the sea luminous.

" Such are the essential and general facts which seemed to me to result from the examination of the particular facts, of which the History of Electricity, Galvanism, and Magnetism is composed. They are immediately connected with the universal system, the principle of which I have presented to the public *. They are demonstrated by this principle, and, in their turn, they serve to elucidate this principle itself.

" The universe is an assemblage of facts, directed and tied together by one sole cause.

" I shall soon explain what I have been able to discover of this magnificent assemblage."

XXVI. *Proceedings of Learned Societies.*

THE IMPERIAL ACADEMY OF SCIENCES AT ST. PETERSBURGH.

THE above Academy had, in their last public notice, proposed the prize of five hundred roubles to be given to any professor of physic, who would establish, and communicate to the academy, " a series of new and instructive experiments on light, considered as matter, also on the properties which may in part be attributed to it; on the affinities which it may appear to have, either to organized or inorganized bodies, and upon the modifications and phænomena of these substances, by their combinations with the matter of light." The academy had declared at the same time, in order not to confine the learned, who might have similar pursuits, that they contented themselves with stating the subject generally, leaving them at liberty to con-

* The author here alludes to a work he has just published under the title of *Essai sur le Monde*.—EDIT.

sider

sider the question in any point of view that might appear the best calculated to elucidate the access to a question so difficult. The academy has received, within the prescribed time, six tracts on the question, each having a note sealed, and a motto ; viz.

No. 1. In the Russian language with the motto, "*A philosopher who has learn'd to doubt, knows more than all the learned,*" &c.

No. 2. In the Russian language, "*Time is the earliest thing in nature,*" &c.

No. 3. In Latin, "*Est-ne color proprius verum, lucisne repulsus eludunt aciem?*"

No. 4. In French, "*Nox abiit, nec tamen orta dies!*"

No. 5. In German, "*Ut noscas splendore novo res semper egere, et primum jactum,*" &c.

No. 6. In German, "*La physique ne sera véritablement une science, que lorsque tous les effets naturels se déduiront clairement d'un seul et même principe évidemment démontré.*"

The three first tracts, (No. 1. 2. 3.) beside the common fault of wanting new experiments, a complete and instructive series of which was required by the academical notice, contained hypotheses and propositions, either well known, erroneous, or ill expressed, and advanced without demonstration: for these reasons, the academy did not think these tracts could aspire to the prize.

The tract No. 4. is not without merit : the author enters upon several interesting questions concerning the nature of light, in a manner that readily convinces us he is not a stranger to the subject ; but the deficiency of connexion and systematic arrangement, which is perceived in the tract, and above all the absolute want of new experiments, which might lead to new results, or serve as a support to a number of hypotheses advanced by the author, and destitute of every species of demonstration, would not permit the academy to adjudge the prize to the memoir, even had there been none of greater merit.

As to the last pieces, (No. 5. and No. 6.) the academy has found them worthy of particular attention, from the report of the committee appointed to declare the best qualified performance.

formance. These essays (No. 5 and No. 6.) are agreeable to the principal condition stated in the notice, inasmuch as they contain a great number of new experiments on the effects and properties of light, and a judicious application of those which. though already known, were repeated whenever they appeared to the author doubtful. Both pieces are executed upon a plan wisely conceived, expressed with clearness, and arranged in a sufficiently systematic order. On the other hand, in each were found some incoherent and contradictory conclusions, as also propositions hazarded without sufficient proof, besides some errors and obscure passages: but as these imperfections were overbalanced by researches of great merit, the academy, without acceding to every assertion of the authors, have nevertheless thought it their duty to divide the prize between the authors of No. 5. and No. 6. thinking them worthy of encouragement and honourable reward.

On opening two of the sealed notes, doctor Henry Frederick Link, professor of physic at the university of Rostock, was found to be the author of No. 5. and Mr. Placidus Heinrick, professor of physic and mathematics to the Abbey de St. Emeran, at Ratisbon, the author of No. 6. The notes of the remaining tracts were burnt without being opened.

When the academy made public the notice in which the marine department proposed a prize on the question concerning the resistance of fluids, they engaged to publish also the judgment which that department, in conjunction with the academy, should make on the memoir presented conformably with this engagement. The academy announce by the present, the receipt of these memoirs: viz.

No. 1. with motto, "*Sit modus lasso maris et viarum militiæque.*"

No. 2. "*Præstat naturæ voce doceri, quam ingenio suo sapere.*"

No. 3. "*England and France agree.*"

(The last of which came after the term).—None were found to satisfy all the conditions of the problem; but as the tract No. 2. exhibits a new theory, which, though not established

upon

upon grounds sufficiently solid, nor applied to naval architecture in the manner the notice required, yet is preferable, in some measure, to the theories of Rome, and of don George Juan; agrees better with experiments than the common theory, and deserves therefore to be noticed advantageously: the marine department, to recompense the author for his trouble and laudable efforts, have decreed to him the prize of 100 Dutch ducats, and the academy have given their sanction to this decision. The opening of the sealed note discovered the author in the person of Mr. Zacarie Nordmark, professor of mathematics in the university of Upsala. In publishing these judgments, and distribution of prizes for the year 1806, the academy proposes the following question for the present year 1807:

Chemistry teaches us the means of discovering the noxious qualities of mineral bodies, whereas it is only by empiricism that we have learned to distinguish noxious plants from those that are not so; even the characteristics by which we think ourselves enabled to determine on the presence or absence of poison in vegetables are not always sufficiently certain and incontestable.

The livid colour, for example, which has rendered many plants suspected, is a deceiving sign. The bur (*Arctium Lappa*) looks dull, and is of a pale colour, yet it is a wholesome plant; on the contrary, the laurel (*Daphne*) is remarkable for the beauty of its flowers and leaves, yet this is poisonous. The families of *Ranunculus* and *Anemone* are as beautiful as they are numerous, they are however for the greater part noxious.

The same may be said of the disagreeable smell of plants, which is taken for a diagnostic of the poisonous quality, and which sign is equally uncertain with the preceding.

The colour of the laurel is very agreeable, while the orache (*Chenopodium vulvaria*), an innocent and even salutary plant, is of a very disagreeable smell. The odour of the coriander is disagreeable to many persons, yet of a very salutary nature.

The umbelliferous plants which grow in damp and inundated situations, have the reputation of being poisonous: not-

notwithstanding this, the *Suine* (la Berle) and all its species; the *Sison inundatum* and *salsum*; the *Phellandrium aquaticum*; the *Angelica sylvestris*; the *Ægopodium podagraria,* plants which thrive in marshes, contain no poison.

It is plain therefore, that neither the pale colour, disagreeable smell, or growth in marshy places, can furnish us with certain and indisputable signs of the presence of poisonous qualities in plants.

The pretended repugnance of animals to pernicious plants is evidently as little infallible. The division of plants made by botanists into classes, orders, and families, according to their nature, is not more efficient in recognising those that are venomous. To be convinced of this, we have only to observe, that among the principal genus of the nightshade, so suspected, is found the potatoe (*Solanum tuberosum*), and also capsicum (*le Piment des jardins*), which possesses the virtue of exciting and destroying the pernicious principle in narcotic plants.

In consequence of this want of an exterior and natural certain sign, by which noxious plants might be immediately detected, it would be desirable to find out some easy method of examining them; such for instance as an *Eudiometre,* or any thing that might produce changes in them which (like the black colour assumed by mushrooms when they are boiling) might indicate their noxious qualities, though the criterion of poisonous mushrooms is not yet sufficiently established.

"*An easy method is therefore required, by which any individual, not having the least knowledge of botany, may detect venomous plants, in a short time, at a small expense; and in a manner perfectly decisive.*"

The prize is one hundred Dutch ducats; and the precise time, after which no memoir can be admitted to the competition, is the 1st of July 1808.

The academy invite the learned of all nations, without excluding its honorary members and correspondents, to investigate this matter. None are excluded but those academicians who are to exercise the function of judges.

The learned who contend for the prize must not put their

their names to their works, but merely a sentence, or motto, with sealed notes added to them, having on the outside the same motto, and the author's name, quality, and place of residence within. The note of the piece to which the prize is adjudged shall be opened, and the rest shall be burnt unopened.

The tracts, legibly written, may be either in Russian, French, English, German, or Latin, and must be addressed to the permanent secretary of the academy, who shall deliver to any person, the author may appoint, a receipt, marked with the same number and motto marked on the piece.

The successful memoir is to be the property of the academy, and without whose formal permission the author shall not print it: the rest of the tracts may be received back from the secretary, who will deliver them at St. Petersburgh to any person commissioned by the author to apply for them.

PHILOSOPHICAL SOCIETY OF LEIPZIC.

The above society offers a gold medal of the value of 24 ducats, for the best memoir "Upon the heat of light, liberated by a strong and rapid compression of the air, in which memoir must be collected the phænomena relative to the question: these phænomena must also be explained, and the consequences drawn from them."

The memoir must be transmitted to the society on or before the 1st of January 1808.

XXVII. *Intelligence and Miscellaneous Articles.*

THE LANDSDOWN MANUSCRIPTS.

We are happy to inform our philosophical readers, that this valuable collection of antient records is about to be added to the treasures already in the British Museum. The house of commons has voted the sum of 4,925*l.* to the executors of the late marquis of Landsdown, as the price of these manuscripts.

FREEZING

FREEZING OF MERCURY.

Some experiments have been made at Hudson's Bay with frozen quicksilver; it has been reduced to sheets as thin as paper, by beating it upon an anvil, with a hammer, both at the same temperature as quicksilver.

On plunging a mass of frozen quicksilver into a glass of warm water, the latter was immediately frozen, the glass was shivered into a thousand pieces, and the quicksilver became fluid again.

MONNIKHOFF'S MEDAL.

The trustees for distributing the prizes bequeathed by the late M. Monnikhoff, of Amsterdam, have offered a gold medal, of 300 ducats in value, for the best memoir upon umbilical hernia. The memoir may be written in Latin, French, Dutch or German.

M. CHATEAUBRIANT.

Intelligence has been received at Paris from Constantinople, that this indefatigable traveller, after having traversed Syria and Egypt, had arrived in good health at Tunis. The ruins of Carthage had attracted M. Chateaubriant's particular attention. From Tunis he was to embark for Spain, where his friends hope to receive very soon the intelligence of his safe arrival.

LIST OF PATENTS FOR NEW INVENTIONS.

To John Syeds, of Rotherhithe Wall, in the county of Surry, compass-maker; for certain improvements in the construction of a machine for making rope or cordage, either shroud or cable laid, and in the mode of manufacturing the same. June 16.

To Robert Barlow, of Spring Gardens, in the county of Middlesex, chemist and medical electrician; for certain oriental aromatic chemical compositions or compounds to be made and moulded into various forms, shapes, and ornamental devices, as amulets, in butterflies, birds, shells, and animals; and to be worn as an ornamental part of dress

by

by ladies and gentlemen, as rings, broaches, lockets, pins, combs, bandeaus, and other ornaments; which oriental aromatic chemical composition he denominates ebenbosamic and ebengarric bosamic composition or compounds, or aromatic variegated artificial marbles and stones opake and transparent. June 16.

To William Atkins, of the city of Norwich, shawl-manufacturer; for certain improvements in the construction of a loom for weaving borders or stripes, or different co ours, on shawls or any goods made of cotron, silk, linen, or worsted, or any mixture of the same. June 16.

To John Palmer, of Enon Cottage, Shrewsbury, in the county of Salop; for a new method of constructing and erecting bridges. June 26.

To John Diekinson, of the parish of St. Martin Ludgate, in the city of London, stationer; for a machine or machinery for cutting and placing paper. June 30.

To William Bound, of Ray-street, in the parish of St. James, Clerkenwell, in the county of Middlesex, smith and iron founder; for a receiver applicable to register and other stoves, by which means the cinders and ashes are, with cleanliness and safety, constantly retained, while the same forms an easy support to a general fire-screen. July 4.

To Apsley Pellatt, of St. Paul's Church-yard, in the city of London, glass manufacturer; for his improved method for admitting light into the internal parts of ships, vessels, buildings, and other places. July 7.

To Charles Groell, of Leicester-Fields, in the parish of St. Martin and city of Westminster; for certain improvements on harps. July 13.

To John Norton, of Rolls Buildings, Fetter-Lane, in the city of London, mathematical instrument maker; for an improved pump. July 13.

To James Bradly, of Maid-Lane, Southwark, in the county of Surrey, iron founder; for a new kind of iron bar to be used in fire-places, for boilers, furnaces, hot-houses, and any other fire-place where bars are used. July 13.

METEORO-

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For July 1807.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
June 27	64°	76°	61°	30·02	52	Fair
28	60	70	55	·05	51	Fair
29	60	69	54	29·95	45	Fair
30	55	63	58	·92	40	Cloudy
July 1	57	61	54	·95	32	Cloudy
2	60	66	55	·92	35	Cloudy
3	58	65	56	·92	38	Cloudy
4	57	67	59	30·00	35	Cloudy
5	59	69	59	·05	50	Fair
6	60	67	54	·01	45	Fair
7	56	63	53	·22	45	Cloudy
8	55	67	55	·28	50	Fair
9	56	74	64	·23	82	Fair
10	65	78	67	·04	84	Fair
11	67	76	64	29·70	45	Fair
12	67	74	69	·85	46	Fair
13	70	77	68	·93	50	Fair
14	64	66	65	·96	10	Rain
15	64	74	61	·89	57	Cloudy
16	63	73	64	·98	47	Cloudy
17	64	74	65	30·01	57	Fair
18	66	73	66	·10	51	Fair
19	67	74	67	·10	52	Fair
20	68	80	64	29·95	57	Fair
21	66	76	67	·95	65	Fair
22	68	82	70	·78	56	Fair
23	69	76	68	·73	47	Fair
24	68	76	64	·69	42	Showery
25	66	73	64	·90	47	Cloudy
26	69	77	62	·78	55	Fair

N. B. The Barometer's height is taken at one o'clock.

Erratum, vol. xxvii. p. 322.—Mr. Kater's communication: for "The following paper was," &c. *read* "The paper, of which the following is a part, was," &c.

XXVIII. *Essay upon the Art of the Foundry among the Antients: with some Remarks upon the celebrated Horses of Chio, now brought from Venice to Paris.* By M. SEITZ*.

History of the Art among the Assyrians, the Etruscans, and the Greeks.

THE art of working metals must have had its rise in countries where the heat of the sun produces them in abundance. If the account given by Philostratus † of a table or bas-relief of bronze, which Apollonius found in a temple near Taxilla, is not exaggerated, the Indians must have carried this art to the highest possible degree of perfection. We there see represented the combats which Alexander fought with the army of Porus. The different metals were there employed and mixed, so as to form the effect of colours ‡, and this bas-relief might be compared with the finest pictures of Euphranor and Polycletes. This kind of mosaic work in metals cannot be referred to a more antient date than the days of Alexander; but the finished manner in which the works of that period are executed, must have been the result of several centuries of experience §; and the abundance of metallic substances which under a burning soil we often find pure and without mixture, must have induced the most antient nations to practise the art of the foundry at an early period. The table of Isis, the blackish ground

* From *Mag. Ency.* for December 1806.

† Life of Apollonius of Thyana, book ix. ch. 20.

‡ According to Pliny, the metal founders imitated the purple with which the toga pretexta was edged, by mixing lead with Cyprus copper. Lib. xxxiv. ch. 40.

Aristonides expressed the remorse felt by Athamas for having crushed his son Leauchus against a rock, by mixing iron with bronze, so that the shame which suddenly succeeds fury was perfectly well expressed. This statue was to be seen at Thebes.

§ I am inclined to think, with the learned author of this article, that the art of melting metals is very antient in India; but I do not think the monument he refers to is any proof of it: it was, as he himself says, of the days of Alexander, but, in addition to this, it was certainly executed by a Grecian artist.—*Note by M. Millin, Editor of the Mag. Ency.*

of which is a mixture of several metals, and of which all the designs are pieces of silver hardened, gives us an idea, although very imperfect, of a similar work.

The Assyrians and the Babylonians were very dextrous in casting metals. Semiramis * ornamented her gardens with bronze animals of every description; and in the temple of Belus were seen statues in gold of Jupiter, Juno, and Rhea †, of an enormous size, and wrought with the hammer. An attempt has been made to throw some doubt upon the recital of Diodorus ‡, who relates all these wonders upon the faith of Ctesias, whose authority is much questioned: but if we reflect, that in these antient times the gold of all Asia was collected in the single city of Babylon; if we believe the Bible §, which says, that from the time of Solomon gold became so common in Jerusalem that silver lost its value; that Nebuchadnezzar, in short, erected a statue of gold sixty cubits high and six broad in the plain of Dura, near Babylon ||, we shall find the report of Diodorus by no means exaggerated. It may be asked, if elegance and correctness reigned in these works? This may be doubted with so much the more propriety, that the ruins of Persepolis do not give a favourable opinion of the taste or precision of the Asiatics in the arts of design ¶.

Hiram, the Phœnician, is the most antient founder whose name has been handed down to us in history. Solomon brought him from Tyre **, in order to make the works in bronze which were to decorate the temple of Jerusalem. He made two columns in bronze, each of them 18 cubits high (31 feet, taking the cubit at 1 foot nine inches); with two

* She reigned 1740 years before the vulgar æra.

† The names of these Greek divinities must have been unknown to the Assyrians at that period. What Diodorus Siculus relates of Semiramis may justly be regarded as fabulous.—*Note by M. Millin.*

‡ Book ii. 59.

§ Kings, book ii. ch. 9. See Calmet's Dissertations upon the Riches left by David to Solomon: Commentaries upon the Bible, vol. ii. p. 155.

|| Daniel, ch. iii. ver. 1. He reigned 606 years before the vulgar æra.

¶ Goguet de l'Origine des Lois, des Arts, et des Sciences.

** A. 1015 before the vulgar æra.

capitals

capitals of 9 feet in height, which were surmounted by pomegranates 7 feet high; so that each column was 47 feet high in all *.

But the performance which surpasses all that Herodotus, Diodorus and Pausanias relate of the antient workmen, was the brazen sea, executed by the above artist. Twelve oxen, of the natural size, supported an enormous bason 18 feet in diameter by nine in depth; it rested upon ten brass sockets furnished with wheels, by means of which the whole machine could be put in motion. Between the oxen there were cherubims which supported the edges of the bason. The description, given in the third book of Kings, of all the ornaments which adorned this work, proves that this Phœnician artist had some taste, and that a similar work could hardly be executed at present with more elegance †.

In order to mould and cast this sea of brass, columns, and several other works, the king assigned to Hiram a plain near the Jordan where there was plenty of clay: this was the only substance which was necessary for him; in those days wax was not used.

From the time of Homer ‡ the scarcity of iron was the reason why warlike nations refined upon the improvement of the other metals. The wheels of the chariots upon which they fought were cast in bronze; the blades of their swords and the points of their spears were of copper, to which they knew how to give, by tempering, such a degree of hardness that it was equal in every respect to iron. But the artificial casting of statues was only known in Asia Minor; and

* The shafts of these columns were 12 cubits in circumference, and consequently 4 cubits, or 7 feet, in diameter; they are therefore, at the utmost, not more than 4 diameters and a half in height. This enormous size does not answer to any Grecian order; but the columns which have been found in the Thebaid prove distinctly that Hiram conformed himself to the proportions of the Egyptian architecture.

† It is astonishing that the author of this interesting memoir does not take notice of the most antient works of the Hebrews; such as the making of the breastplate of the high priest, the melting of the golden calf; which prove that the Hebrews had acquired this art while among the Egyptians, where it was known at a very remote period; and yet M. Seitz does not speak of the Egyptians at all.—*Note by M. Millin.*

‡ 950 years before the vulgar æra.

Greece,

Greece, properly so called, was not as yet sufficiently civilized to pay any attention to the arts which flow from luxury. Homer, who wrote in Asia Minor, could not know any thing of the buckler of Achilles, except from some models which must have resembled it. When he describes the palace of Alcinous *, he speaks of two dogs, one of gold and the other of silver, which Vulcan had presented to this prince. He places in the same edifice statues of gold representing young boys holding lighted torches; but it is in Asia he places all this; and when he describes the palaces of the Greek princes, we see plenty of riches, but no imitative work †.

It was from the Lydians and the Phrygians that the Greeks of Asia Minor received the art of melting and working in bronze, as well as the musical instruments of the Lydian fashion, and their manufactures. The works still shown of the time of Herodotus ‡, at Delphos, are proofs of this. According to him, Midas, king of Phrygia, sent to the Delphic Apollo a throne, probably of bronze, or some other precious metal, which was remarkable for workmanship; and Gyges, king of Lydia, consecrated to the same god six goblets of gold weighing thirty talents, and probably made in the country. From Asia Minor this art penetrated into the islands of the Ionian Sea; and the first founders in metal known by the Greeks were inhabitants of the islands of Samos and Chio.

While Greece was torn with factions and intestine wars, the Etruscans made great progress in the melting of the metals. The most antient statues of these people have their arms resting on their sides, like those of the Egyptians §; which inclines us to think they received their first arts and their theology from the Egyptians and the Phœnicians, with whom they traded. They lived, moreover, in a state of calmness and tranquillity, which lasted to the time of the prepon-

* Odyssey, book vii. ver. 92 and 100.

† I think it is going too far to assert that the art of executing works of imitation was unknown in Greece at the time of Homer.—*Note by M. Millin.*

‡ Book i. ch. 14.

§ Gori, *Museum Etruscum.*

preponderance of the Romans, and which was very favourable to the arts. On this account the whole of Italy was covered with Etruscan images before the Greeks had produced any thing remarkable in the art of the foundry *. In the eighth Olympiad, Romulus placed his statue, crowned by Victory, upon a chariot, to which were yoked four bronze horses which had been carried off from the city of Cameria†. It was an Etruscan performance; and the seven statues of the kings of Rome, which were still seen in the time of Pliny ‡ upon the Capitol, were also cast in bronze by artists of this nation.

Between the 30th and 40th Olympiad, Rhœcus and Theodorus practised the art of casting metals with much success at Samos §; but they wrought for Asia Minor only; and

* The general opinion at present, however, is, that it was the Greeks who brought the arts into Italy, and that the works said to be Etruscan belong to the antient Grecian style. But the inhabitants of several countries of Italy, and particularly the Etruscans, preserved and practised the arts a long time before the Romans paid any attention to them.—*Note by M. Millin.*

† Dionysius of Halicarnassus. *Roman Antiq.* book ii.

‡ Book ii. ch. 17.

§ Herodotus, book iii. § 60, says they invented the art of making moulds with clay; and Pausanias, book viii. ch. 14. thinks they were the first who melted statues at one single cast. To this M. Meyners answers,—"It is not true that Theodorus and Rhœcus were the inventors of casting in brass; what appears more probable is, that having surpassed other artists in statues and other works in bronze, the time at which they flourished has been fixed upon as the æra of the renovation of the fine arts." See Meyners, *Histoire de l'Origine, des Progrès et de la Décadence des Sciences dans la Grece*, p. 39.

After all, we cannot ascribe to them the honour of an art which 400 years before was practised, in all its perfection, by the Phœnicians. Pliny, book xxxiv. ch. 8. speaks of a statue cast by Theodorus, which represented him holding in his left hand a small *quadriga*, so delicately and finely wrought that a common fly could cover it with its wings. It seems at first view singular that a founder, accustomed to work upon a large scale, should occupy himself with a work the only merit of which seems to be the patience bestowed upon it; but Theodorus was anxious to prove his talent by imitating nature in small. He was an engraver upon fine stones, and he engraved an emerald for Polycrates of Samos.

The stones worn in rings were an object of luxury; the more figures the engraver was able to bring into a small space, the higher was his price. It was thought for a long time that these engravings were executed without the assistance of the microscope; but some lenticular glasses found in the ruins of Herculaneum, and which are to be seen in the Royal Museum at Portici, prove

and it is not probable that their works penetrated into Greece properly so called, because Pausanias assures us, that, although no work in bronze had escaped his attention, he had not found any thing belonging to Theodorus, and that he had only seen a single statue of Rhœcus in the temple of Diana at Ephesus, in Asia Minor.

The European Greeks were as yet so little advanced in the arts, that the conquerors at the Olympic games had not statues assigned them until the 61st Olympiad, and they were then of wood only; but twenty years afterwards one Cleostenes, an obscure individual, who had vanquished in the 66th Olympiad, caused himself to be represented in a chariot with four bronze horses; and, not content with engraving upon it his own name and that of his squire, he inscribed the names of his horses also.

It was from this period that the sacred grove of Altis, near Olympia, was filled with a number of bronze statues, in order to do honour to the memory of the conquerors, while

prove that the antients knew the use of dioptric glasses. See Dutens, *Origine des Découvertes attribuées aux Modernes*, tom. ii. ch. 10. § 278.

Fashionable people of the present day wear upon their fingers a large diamond the lustre of which is often equalled by a piece of well polished glass, and completely outvied by the dew drops when the sun shines upon them; while the antients employed a part of their riches in the purchase of rings with a precious stone in them representing the battles of the Centaurs, or Ulysses traversing with Diomedes the Trojan fields in order to carry off the Palladium. In laying out their money they recompensed an industrious artist, whose work astonishes the eye by its minuteness, and recalls to memory an interesting history or a tradition consecrated by antiquity; and thus contributed at the same time, by the solidity of the materials, to the instruction of the most distant posterity. If our descendants could dig up one of our porcelain vases two thousand years hence, what instruction would it not afford them!—*Note by the Author.*

In adopting the principal ideas consigned in this curious note by the author of this dissertation I think, however, that some of his assertions are not well founded. At first the antients did not endeavour, as he says, to bring the greatest number of figures possible into a small space. On the contrary, they took care not to multiply the figures, that they might not divide the attention too much. This is demonstrated by the great number of monuments which have been handed down to us: one of the proofs which may incline us to regard as modern the celebrated intaglio known by the name of Michel Angelo's seal, is the great number of figures with which it is loaded.

In my opinion, it is by no means demonstrated that the antients were acquainted with magnifying glasses.—*Note by M. Millin.*

Pindar

Pindar immortalized them by his poems *. In order to distinguish themselves, kings, cities, and some rich individuals, who had gained prizes at the Olympic games, caused themselves to be represented in a car of bronze drawn by two or four horses of the same metal.

This new branch of luxury exercised and favoured the rising talents of the statuaries: by degrees the art of founding rose to the highest pefection, and the renown of the artists far surpassed that of the candidates in the race, or the athletæ whom they represented. It will not be extraneous to enumerate here those who distinguished themselves in the art of making horses and chariots of bronze.

Account of the celebrated Sculptors who have founded Bronze Horses and Quadrigæ.

The most antient we know is Glaucias, of Æginus; he flourished in the 72d Olympiad †, and made a car with four horses, with which Gelon, king of Syracuse, ornamented the wood of Altis, near Olympia, for having carried off the prize in the race.

Onatas of Æginus, and Calamides, cotemporaries of Glaucias, both laboured at a chariot with four horses, which king Gelon, brother and successor of Hieron, erected at Olympia for having obtained a similar victory. These two chariots were still in their place at the time of Pausanias, who wrote in the reign of Marcus Aurelius.

Polycletes ‡ perfected the art invented by Phidias. His human statues were almost all placed in such a manner, that one of the two feet always supported the whole weight of the body, and the other foot seemed at ease: by this improvement his works had less regularity and more grace.

* Pindar sold his poems at a very high price. One Pytheas having carried off the prize of Pancrace at the Nemean games, his fellow citizens, who shared his glory, were divided upon the question whether to erect a statue to the juvenile victor, or to employ the same expense in order to celebrate his victory by a poem of Pindar's. The latter alternative was adopted; and Pindar, highly gratified with this preference, begins his fifth Nemean ode with expressing his sense of the honour thus conferred upon him.

† 490 years before the vulgar æra.

‡ 87th Olympiad, 430 years before the vulgar æra.

Nevertheless all his figures were robust (quadratæ), and rather had the appearance of strength than of elegance. He has been reproached with too much uniformity in his manner; one single statue gave an idea of all the rest*. He was not acquainted with the art of making horses †.

Myron ‡ had more invention than Polycletes, and his performances had more diversity. His imagination, however, was rather displayed in the different attitudes of the body than in the movements of the mind; he did not express the passions, and did not know how to make horses any more than his predecessors. We have still a copy of his Dioscobulus. His statue of Leda, who had gained the prize of the race in the Olympic games, was the subject of several epigrams which we find in the Greek Anthology §.

Aristides, a statuary of the 89th Olympiad, made some quadrigæ. Euphranor, of Corinth, was also celebrated in this branch of workmanship ||.

Hypatodorus and Aristogiton were statuaries of the Athenian school, and contemporaries with Praxiteles. They made the chariot of Amphiaraus, in which we see Laton, his charioteer. This monument was in existence at Delphos in the time of Pausanias.

Calamis ¶, contemporary with Praxiteles, excelled in moulding horses; and no person disputed this talent with him **. Praxiteles, on one occasion, made for this artist a leader for one of his chariots. Thus Poussin frequently took pleasure in ornamenting the landscapes of Guaspre with several figures.

Lysippus, of Sicyone, was contemporary with Alexander ††. His fertile genius produced more works than any

* Pliny, book xxxiv. ch. 8. *Pene unum ad exemplum.*

† That is to say, he succeeded less in this than in the other branches.

‡ 87th Olympiad.

§ Anthologia, book iv.

|| Pliny, lib. xxxv. ch. 14.

¶ 107th Olympiad.

** *Equis semper sine æmulo expressis.* Plin. *Exactis Calamis se mihi jactat equis.* Propert. Eleg. i. ver. 33. *Vindicat ut Calamis laudem quos facit equorum.* Ovid. de Ponto, lib. iv.

†† 114th Olympiad A. 320 before the vulgar æra.

ten

ten others. He made several quadrigæ, a quantity of equestrian statues, and decorated, not only Greece and Macedonia, but also the Greek cities of Italy and Sicily with his chef d'œuvres. He gave the expression of his horses with great fidelity, and he observed it in the minutest detail *. He displayed this talent principally in the manes of his horses, which he represented at full length and waving in the air, as may be seen in an epigram made by Philip of Macedon on the subject of one of his horses †. He gave more roundness to the forms, and less acuteness to the muscles; and represented the limbs as more at ease. On comparing his works with the clumsy and fleshy statues of his predecessors, we might say that he has invented new proportions for the human body ‡. In fact, he made the heads smaller, and the limbs more graceful. By this means his statues appeared larger; and it was generally said of him, that if the others had represented men such as they really were, Lysippus would have raised nature above herself. We may easily imagine he introduced the same changes in the designs of his horses, and that his labours of this description must have been superior to any thing seen before his time.

Euthycrates §, his son and pupil, made several quadrigæ representing the chariot of Medea. He abandoned the elegance and softness of his father, and substituted in place of it a more noble and masculine style.

Pyromachus ||, of the school of Lysippus, made a chariot with four horses carrying Alcibiades in it. This work was at Olympia; but it was carried off and brought to Rome by Nero ¶.

The works of all these artists were either among the monuments of Delphos and Olympia, or dispersed in the other cities of Greece to the time of Nero, who carried off the greatest part of them in order to adorn his golden palace and

* Propriæ hujus videntur esse argutiæ operum, custoditæ in minimis quoque rebus.

† Anthol. lib. iv. ch. 7.

‡ Novâ intactâque ratione quadratas veterum staturas permutando.

§ 119th Olympiad.

|| Ibid.

¶ Plin. lib. xxxiv. ch. 8.

other

other edifices at Rome. Under Vespasian they became the ornaments of the Temple of Peace, and of the other numerous edifices of that emperor at Rome *. Notwithstanding all this, Greece was not yet exhausted; at the time of Pliny 3000 statues were counted in the island of Rhodes alone, and an equal number in the three cities of Athens, Delphos, and Olympia.

The instant a stranger arrived at Delphos, the antiquaries of the city presented themselves to him in great numbers, for the double purpose of showing him all the wonders of art, and of explaining to him the number of inscriptions with which they were loaded.

Plutarch † relates, that, being there with some strangers, the demonstrator expatiated with so much verbiage upon all the inscriptions, that, the day beginning to decline, they requested him to be less prolix, and to dwell upon the most important monuments only.

One of the strangers, who seemed to be a connoisseur, and had a very accurate eye, was quite astonished at the art which reigned in most of these statues: he was particularly surprised at the colour and the lustre of the most antient monuments, which represented heroes who were victorious by sea. Neither rust nor verdigrise was to be seen upon them; but the colour of the bronze was blueish, and resembled the element which was the theatre of their exploits. Of what mixture of metals did the antients make use, says he, to produce so fine a colour, joined with so much brilliance? The dialogue afterwards turned upon the bronze of Corinth. It was agreed that it was nothing else than a fortuitous mixture occasioned by the burning of that city at the time of its capture by Mummius; that the bronze, silver, and gold, melted together, had produced that composition, which, because the bronze was most abundant, retained the name of Corinthian bronze; that the gold and silver mixed produced a paleness which was not very agreeable to the eye; and, in short, that this blueish colour of the bronze of Delphos was the effect of the air which pe-

* Plin. † Cur nunc Pythia non reddat oracula.

netrated

netrated the pores of the bronze and preserved it from rust. Naturalists will soon see if this reasoning, which seems satisfactory to Plutarch and his contemporaries, can be admitted at present.

The time of Alexander was the epoch at which the art of founding flourished most in Greece. The Athenians erected to Demetrius Phalereus, who governed Athens for ten years, 360 bronze statues, several of which were equestrian, or accompanied with chariots and horses; and, what appears almost incredible, all these works were commenced and finished in the space of 300 days*. Lysippus seems to have exhausted this art, for it fell with him.

[To be continued.]

XXIX. *An Account of two Children born with Cataracts in their Eyes, to show that their Sight was obscured in very different Degrees; with Experiments to determine the proportional Knowledge of Objects acquired by them immediately after the Cataracts were removed.* *By* EVERARD HOME, *Esq. F. R. S.*

MR. CHESELDEN's observations on this subject, recorded in the Phil. Trans. for the year 1728, pointed out two material facts; that vision alone gives no idea of the figure of objects, or their distance from the eye, since a very intelligent boy, 13 years of age, upon recovering his sight was unable to distinguish the outline of any thing placed before him, and thought that every object touched his eye.

Mr. Ware's cases, which have also a place in the Phil. Trans. for 1801, and are compared with that of Mr. Cheselden, appear to lead to a different conclusion. The following observations are laid before the society with a view to explain this circumstance.

Case I.

William Stiff, twelve years of age, was admitted into St. George's Hospital, under my care, on the 17th of July 1806, with cataracts in his eyes, which, according to the

* Diogenes Laërtius, lib. v. ch. 11.

account

account of his mother, existed at the time of birth. From earliest infancy he never stretched out his hand to catch at any thing, nor were his eyes directed to objects placed before him, but rolled about in a very unusual manner, although in other respects he was a lively child. The eyes were not examined till he was six months old, and at that time the cataracts were as distinct as when he was received into the hospital.

Previous to an operation being performed, the following circumstances were ascertained respecting his vision. He could distinguish light from darkness, and the light of the sun from that of fire or candles: he said it was redder, and more pleasant to look at, but lightning made a still stronger impression on his eyes. All these different lights he called red. The sun appeared to him the size of his hat. The candle flame was larger than his finger, and smaller than his arm. When he looked at the sun he said it appeared to touch his eye. When a lighted candle was placed before him both his eyes were directed towards it, and moved together. When it was at any nearer distance than 12 inches, he said it touched his eyes. When moved further off he said it did not touch them; and at 22 inches it became invisible.

On the 21st of July the operation of extracting the crystalline lens was performed on the left eye. The capsule of the lens was so very strong as to require some force to penetrate it. When wounded, the contents, which were fluid, rushed out with great violence. Light became very distressing to his eye, and gave him pain. After allowing the eye-lids to remain closed for a few minutes, and then opening them, the pupil appeared clear, but he could not bear exposure to light. On my asking him what he had seen, he said, "Your head, which seemed to touch my eye:" but he could not tell its shape. He went to bed, and took an opiate draught: the pain in his eye lasted about an hour, after which he fell asleep. The whole of that day the light was distressing to his eye, so that he could not bear the least exposure to it.

On the 22d the eye-lids were opened to examine the eye. The

The light was less offensive. He said he saw my head, which touched his eye. There was so much inflammation on the eye-ball, that a leech was applied to the temple, and the common means for removing inflammation were used.

On the 23d the eye was less inflamed, and he could bear a weak light. The pupil was of an irregular figure, and the wounded cornea had not united with a smooth surface. He said he could see several gentlemen round him, but could not describe their figure. My face, while I was looking at his eye, he said was round and red.

On the 25th the inflammation had subsided, but on the 27th returned, and continued, notwithstanding different means were employed for its removal, till the 1st of August, when it was almost entirely gone. On the 4th the eye was apparently so well that an attempt was made in the presence of Mr. Cavendish and Dr. Wollaston to ascertain its powers of vision; but it was so weak that it became necessary to shade the glare of light by hanging a white cloth before the window. The least exertion fatigued the eye, and the cicatrix on the cornea, to which the iris had become attached, drew it down so as considerably to diminish the pupil. From these circumstances nothing could be satisfactorily made out respecting the boy's vision. On the 11th a second attempt was made in the presence of Mr. Cavendish, but the pupil continued so contracted and irregular, and the eye so imperfect in its powers, that it became necessary a second time to postpone any experiments.

On the 16th of September the right eye was couched. This operation was preferred after what had happened to the other eye, in the hope that there would not be the same degree of inflammation, and as the former cataract was fluid, there was every reason to believe that couching would in this instance be most efficacious.

The operation gave pain, and the light was so distressing to his eye that the lids were closed as soon as it was over, and he was put to bed. The consequent inflammation was not severe; but as soon as the fluid cataract, which had been diffused through the aqueous humour was absorbed, the cap-

sule

sule of the lens was found to be opaque, and the sight consequently imperfect. The eyes were not examined with respect to their vision till the 13th of October, during which period the boy remained quiet in the hospital. On that day the upper part of the pupil of the left eye had in some measure recovered its natural state, and had become transparent, but the cicatrix in the cornea was more extensively opaque than before. The light now was not distressing to either eye, and when strong, he could readily discern a white, red, or yellow colour, particularly when bright and shining. The sun and other objects did not now seem to touch his eyes as before, they appeared to be at a short distance from him. The eye which had been couched had the most distinct vision of the two, but in both it was imperfect. The distance at which he saw best was five inches.

When the object was of a bright colour, and illuminated by a strong light, he could make out that it was flat and broad; and when one corner of a square substance was pointed out to him, he saw it, and could find out the other, which was at the end of the same side, but could not do this under less favourable circumstances. When the four corners of a white card were pointed out, and he had examined them, he seemed to know them: but when the opposite surface of the same card, which was yellow, was placed before him, he could not tell whether it had corners or not, so that he had not acquired any correct knowledge of them, since he could not apply it to the next coloured surface, whose form was exactly the same, with that, the outline of which the eye had just been taught to trace.

Case II.

John Salter, seven years of age, was admitted into St. George's Hospital on the 1st of October, 1806, under my care, with cataracts in both eyes, which according to the accounts of his relations had existed from his birth.

After he was received into the hospital, the following circumstances were ascertained respecting his vision. The pupils contracted considerably when a lighted candle was placed

placed before him, and dilated as soon as it was withdrawn. He was capable of distinguishing colours with tolerable accuracy, particularly the more bright and vivid ones.

On the 6th of October the left eye was couched. This operation was preferred to extraction, from a belief that the cataracts were not solid, and as the injury done to the capsule by the operation would be less, there was not the same chance of inflammation, the disposition for which had been so strong in the former case. As the eye was not irritable, and was likely to be but little disturbed by this operation, every thing was previously got ready for ascertaining his knowledge of objects, as soon as the operation was over, should the circumstances prove favourable. The operation was attended with success, and gave very little pain. The eye was allowed ten minutes to recover itself: a round piece of card of a yellow colour, one inch in diameter, was then placed about six inches from it. He said immediately that it was yellow, and on being asked its shape, said, "Let me touch it, and I will tell you." Being told that he must not touch it, after looking for some time, he said it was round. A square blue card, nearly the same size, being put before him, he said it was blue and round. A triangular piece he also called round. The different colours of the objects placed before him he instantly decided on with great correctness, but had no idea of their form. He moved his eye to different distances, and seemed to see best at 6 or 7 inches. His focal distance has been since ascertained to be 7 inches. He was asked whether the object seemed to touch his eye, he said "No;" but when desired to say at what distance it was, he could not tell. These experiments were made in the theatre of the hospital, in which the operation was performed, before the surgeons and all the students. He was highly delighted with the pleasure of seeing, and said it was "so pretty," even when no object was before him, only the light upon his eye. The eye was covered, and he was put to bed, and told to keep himself quiet; but upon the house-surgeon going to him half an hour afterwards, his eye was found uncovered, and he was looking at his bed curtains, which were close drawn. The bandage was replaced, but so

delighted

delighted was the boy with seeing, that he again immediately removed it. This circumstance distressed the house-surgeon, who had been directed to prevent him from looking at any thing till the next day, when the experiment was to be repeated. Finding that he could not enforce his instructions, he thought it most adviseable to repeat the experiment about two hours after the operation. At first the boy called the different cards round; but upon being shown a square, and asked if he could find any corners to it, he was very desirous of touching it. This being refused, he examined it for some time, and said at last that he had found a corner, and then readily counted the four corners of the square; and afterwards when a triangle was shown him, he counted the corners in the same way; but in doing so his eye went along the edge from corner to corner, naming them as he went along.

Next day, when I saw him, he told me he had seen "the soldiers with their fifes and pretty things." The guards in the morning had marched past the hospital with their band; on hearing the music he had got out of bed, and gone to the window to look at them. Seeing the bright barrels of the musquets, he must in his mind have connected them with the sounds which he heard, and mistaken them for musical instruments. On examining the eye 24 hours after the operation, the pupil was found to be clear. A pair of scissors was shown him, and he said it was a knife. On being told he was wrong, he could not make them out; but the moment he touched them he said they were scissors, and seemed delighted with the discovery. On being shown a guinea at the distance of 15 inches from his eye, he said it was a seven shilling piece, but placing it about 5 inches from his eye, he knew it to be a guinea; and made the same mistake, as often as the experiment was repeated.

From this time he was consequently improving himself by looking at, and examining with his hands, every thing within his reach, but he frequently forgot what he had learnt. On the 10th I saw him again, and I told him his eye was so well that he might go about as he pleased without leaving the room. He immediately went to the window, and called

out,

out, "What is that moving?" I asked him what he thought it was? He said, "A dog drawing a wheelbarrow. There is one, two, three dogs drawing another. How very pretty!" These proved to be carts and horses on the road, which he saw from a two pair of stairs window.

On the 19th, the different coloured pieces of card were separately placed before his eye, and so little had he gained in thirteen days, that he could not without counting their corners one by one tell their shape. This he did with great facility, running his eye quickly along the outline, so that it was evident he was still learning, just as a child learns to read. He had got so far as to know the angles, when they were placed before him, and to count the number belonging to any one object.

The reason of his making so slow a progress was, that these figures had never been subjected to examination by touch, and were unlike any thing he was accustomed to see.

He had got so much the habit of assisting his eyes with his hands, that nothing but holding them could keep them from the object.

On the 26th the experiments were again repeated on the couched eye, to ascertain the degree of improvement which had been made. It was now found that the boy, on looking at any one of the cards in a good light, could tell the form nearly as readily as the colour.

From these two cases the following conclusions may be drawn:

That, where the eye before the cataract is removed, has only been capable of discerning light, without being able to distinguish colours, objects after its removal will seem to touch the eye, and there will be no knowledge of their outline; which confirms the observations made by Mr. Cheselden:

That where the eye has previously distinguished colours, there must also be an imperfect knowledge of distances, but not of outline, which however will afterwards be very soon acquired, as happened in Mr. Ware's cases. This is proved by the history of the first boy in the present paper, who before the operation, had no knowledge of colours or distances,

 but

but after it, when his eye had only arrived at the same state, that the second boy's was in before the operation, he had learnt that the objects were at a distance, and of different colours: that when a child has acquired a new sense, nothing but great pain or absolute coercion will prevent him from making use of it.

In a practical view, these cases confirm every thing, that has been stated by Mr. Pott and Mr. Ware, in proof of cataracts in children being generally soft, and in favour of couching, as being the operation best adapted for removing them. They also lead us to a conclusion of no small importance, which has not before been adverted to; that when the cataract has assumed a fluid form, the capsule, which is naturally a thin transparent membrane, has to resist the pressure of this fluid, which like every other diseased accumulation is liable to increase, and distend it, and therefore the capsule is rendered thicker and more opaque in its substance, like the coats of encysted tumours in general.

As such a change is liable to take place, the earlier the operation is performed in all children, who have cataracts completely formed, the greater is their chance of having distinct vision after the operation. It is unnecessary to point out the advantages to be derived from its being done at a more early age, independent of those respecting the operation itself.

XXX. *On the Decomposition of Light into its most simple Elements, being Part of a Work upon Colours.* By C. A. Prieur, *late a Colonel in the Corps of Engineers.*

[Continued from p. 170.]

First I considered, that both the nature and quantity of the red, green, and violet rays, which I suppose to be the sole elements of white light, are absolutely unknown. But I could likewise conceive them transformed into coloured matters of such intensity, or condensation, that the mixture of an equal quantity of each should produce exactly white.

In

In the second place I drew Fig. 2. This consists of three curves nearly circular and alike, described round the dial in the following manner. I first described three equal circles, having their centres in the radii drawn through the divisions of 60, 180, and 300 degrees; and the circumferences of which were targents to the dial at the divisions of 240, 390 and 120 degrees respectively. I then modified each circumference by this law, that, on prolonging the diameters of the dial in every possible direction, the sum of the prolongations of every diameter to the new curve should be a constant quantity. It is easy to understand this second construction, by which it will appear, that the resulting curve differs in fact little from the circular circumference.

Thirdly, I conceived, that all the prolongations of the radii of the dial to the red curve represented each a proportional quantity of my red matter mentioned in the paragraph before the preceding; so that this dial is surrounded by a red crescent to a certain point, whence it decreases according to a given law. We must likewise admit a green envelop, analogous to the preceding, and limited by the curve of that colour; and lastly a violet envelop, within the third curve.

This supposed, if for each point of the dial we make a mixture of colours corresponding to that point, we shall have a series of tints in imperceptible gradation from one to the other; which in tone, place, and every other respect, will be extremely analogous to the colours of the dial, that I had previously traced conformably to the ideas of Newton, and are such, that the union of two diametrically opposite to each other, will every where form a white identically the same.

This is a result which I offer as a further probability greatly in favour of my hypothesis of three colours.

It is true, the dial constructed by the first method differs a little from that by the last, as in this the purest red is somewhat nearer the place of the orange, and the violet nearer that of the indigo. But, beside that this difference is little in itself, it is supported by experience; for the relation of colours in general, and the progress of their ab-

sorption, appear to give some preference to the latter method.

Still I must repeat, that the observations I have here made are only to show the possibility of the thing; the question can be decided only by the direct examination of the rays of light on the spectrum in its simplest state, and this remains for me to give.

As few have the means of procuring this very simple spectrum, and there is some difficulty in applying them, I shall enter into this subject somewhat at large. This I conceive to be the more necessary, as few appear to have repeated experiments of this kind since Newton, at least with due precision. Treatises on optics indeed do not mention this repetition formally, many philosophers having attempted it without success, and others having persuaded themselves a little too hasty, that they had completely succeeded; as Abbé Nollet, for instance, whose name has been quoted as an authority.

I should not myself have had the means I long wished, but from the politeness and enlightened assistance of Mr. Trémery. Fortunately his study was provided with every thing necessary; but I shall first briefly describe the nature of the experiment, and the conditions indispensably requisite.

The business was to repeat the experiment, in which Newton obtained a well defined solar spectrum, the breadth of which, by concentrating the pencil of light, was reduced to $\frac{1}{70}$, or $\frac{1}{72}$ of its length; and which consequently exhibited the homogeneal rays incomparably more distinct from each other than in the common spectrum. Opt. Book I. Part. I. Exp. 11.

I have already hinted above, that the success depends, 1st, in operating on a pencil of light that is very small before it reaches the prism; 2dly, in producing by the prism a considerable dispersion of the coloured rays; and 3dly. in receiving their dissected image on a plane very distant from the point of the angle of dispersion.

But these three conditions are not of themselves sufficient. It is almost impracticable to attain the desired object by

by their concurrence, when the rays arrive at first in parallel directions; still more if they arrive diverging, as they do when a pencil of light is admitted through a simple hole in the window-shutter of a dark room; in which case the sensible diameter of the sun's disk must occasion a divergence of the pencil. There is only one circumstance favourable therefore, that in which the rays may be rendered convergent, without infringing the preceding conditions.

The only method of doing this did not escape the sagacity of Newton. He effected it by placing at a considerable distance from the shutter, and but a little before the prism, a lens of a long focus, which by its position regulated the distance of the plane on which the spectrum was to be received. In this way, and by the assistance of some other precautions, he resolved this grand problem in optics.

The following is the manner in which we proceeded, and its results.

It is not easy to procure a single lens, that shall be capable of giving a focus of ten or eleven feet in the position in which Newton employed it; for several glasses of little curvature, that were lent me as fit for the purpose, were altogether incapable of effecting it. I then imagined, that I might succeed by placing near the shutter an object glass of short focus, to make the pencil very divergent beyond it; and placing at the same time at a sufficient distance, an excellent lens of Mr. Trémery's of five feet focus.

The effect answered our wishes, and in consequence we arranged our apparatus as follows: 1, on the outside of the windows, a plane metallic speculum, to reflect the solar image: 2, an object glass of 87 centimetres (33 inches) focus, distant from the speculum about 24 centimetres (9, 36 inches): 3, a diaphragm, pierced with a hole six millimetres in diameter (2·36 lines,) and at the distance of 11 centimetres (4· 3 inches) from the object glass, to introduce the pencil of light into the room: 4, a lens of 162 centimetres (5 feet, 3 inches focus), placed 32 centimetres (1 foot) from the object glass: 5, at 11 centimetres (4. 3 inches) from the lens a prism of very clear flint glass, with angles of 60°, covered with black paper on each side, ex-

 cept

cept at the place left for the transmission of the rays; this prism being continued so as to be moveable in different directions, as occasion might require: 6, a board covered with white cloth, at the distance of 422 centimetres (13 feet, 8 inches) from the lens. All these were placed, kept, or brought into the proper directions, suited to their several purposes, and to the course of the sun. The place too was so contrived, as to be rendered pretty dark at pleasure. Having taken every possible care in arranging our apparatus, we were able to obtain every day, when it was fine weather, a very simple spectrum for several hours; which was quite sufficient for our various experiments, at some of which Messrs. Berthollet, the father and son, Mr. Laplace, and other gentlemen were present.

The spectrum was very distinctly bounded by two rectilinear, and perfectly parallel sides. Its length was a little more than 24 centimetres (9. 36 inches) Its breadth was $\frac{1}{25}$ of its length, when the aperture in the diaphragm was 6 millimetres (2. 34 lines). Sometimes this was reduced one half, and the contraction of the spectrum was proportional, the breadth then not being more than $\frac{1}{50}$; and lastly, by diminishing the aperture, it was reduced to $\frac{1}{83}$ of the length.

As to the strength of the colours, they were vivid and bright as might be expected. The impression on the eye was such, on account of the narrowness of the image, that at a few metres (two thirds of a foot) from the cloth the spectrum appeared as two straight lines, forming a small angle, the apex of which was at the red extremity, and the base at the violet. On going nearer it appeared a single line. It was the same, if the spectrum were examined from a distance through a glass. This doubling or radiating of the image depends on the conformation of the eye, and is connected with some other phænomena, of which I may hereafter give an account.

The distinction of the colours, and their separation into seven classes, was likewise one of the objects of our inquiry. Though the existence of this distinction was perceived, it must be confessed, that it was not easy to trace all their divisions. I made some attempts to effect it, the narrative

narrative of which I shall pass over for the present; merely observing, that Newton did not make his division on a spectrum thus narrowed, but on one much larger, obtained in the usual way without a lens. Opt. I, part 2, prob. I.

Lastly I shall observe, that the green colour in our spectrum did not extend quite to the middle of its length, whence it followed, that the shades between the green and red were a little shortened, and those of the blue and violet proportionally elongated. These effects were owing no doubt to the nature of the flint glass, of which our prism was made. We had no opportunity of procuring common glass free from streaks. Having tried a hollow prism, formed of glasses joined together, and filled with water; the faces of the glasses occasioned duplications of the spectrum, which rendered it confused; so that we returned to our English flint glass, which, while perfectly void of colour, combined homogeneity of substance, and accuracy of structure, with the finest polish; in short it was to all appearance free from defect.

It now remains for me to speak of the particular experiments on the analysis of colours, which I had long planned.

The reader may recollect, that I had suspected the blue to be merely the result of a combination of green and violet rays; and that in like manner the yellow proceeded only from a mixture of green and red. I reasoned then in this manner: on the supposition that in reality there existed no simple rays of blue, if we prevent the arrival of rays to that part of the spectrum, either by a substance that suffers only the green rays to pass, or by one that allows a passage to the violet only, we shall find beyond these substances only green or violet; otherwise, supposing the blue rays to be simple, they will traverse neither of the substances I have mentioned, as we shall find beyond them nothing but black. We may reason in a similar way with respect to the yellow, which must be subjected to the trial of a red substance and a green.

Thus we must be furnished with three substances coloured in the requisite manner. For the violet I employed an

 ammoniacal

ammoniacal solution of copper, in a phial with plane parallel surfaces: for the green a solution of muriat of copper, in a similar phial: and for the red, either wine of a good colour, or a tincture of cochineal. All these must be sufficiently concentrated, or they will transmit other rays, besides those we have in view. This concentration has the inconvenience of rendering the colour obscure, it is true, and this is some obstacle to their use; but it is the only way in which nature permits us to obtain simple colours, and we must be content with it.

Coloured glasses might be substituted for the red and green liquors; but with respect to the violet I could not procure any, on which I could depend.

Every thing being thus prepared, I made my experiments in concert with Mr. Trémery and Mr. Drappier.

We had a screen, which we could place at will before the cloth opposite the place of the spectrum. In this screen was a small circular hole 3 or 4 millimetres (I. 17 or I. 56 line) in diameter, by means of which we could allow a small coloured pencil to fall on the cloth, while all the rest was dark. We could easily ascertain, that this little pencil, taken successively from the different colours, was simplified as much as possible by refraction; for the circular spot, examined some distance with a prism, was not at all irregular.

We then passed through the small hole a pencil of very decided blue; and in this respect our latitude of choice was great, since in our spectrum the blue had an extent of more than 54 millimetres (2 inches). The blue spot being well formed on the cloth, the green phial was placed before the hole; when the light of the spot was immediately much weakened, and its colour changed to green. On substituting the violet phial in the place of the green, the spot became violet. This experiment was repeated several times, that we might convince ourselves of the fact, and succeeded uniformly.

The trial with the yellow succeeded in like manner; it was changed successively to red and green, according to the substance opposed to the rays.

Another

Another day these experiments were repeated with some little alterations. When the small round image fell upon the cloth, we went behind to look at it; and found that it passed through, appearing on the back of the cloth, which was muslin well stiffened with starch. In this way we could make our experiments more conveniently, as we had only to cover our eye with a coloured substance, and look at the little spot through it. When the spot was formed by blue light, it appeared green, or violet, according to the substance interposed. Through a red substance no light was seen: a proof, that the preceding effect did not arise from white light mixed with blue. If it were viewed through an orange glass, the property of which is to absorb only the blue and the violet rays, the spot appeared green; a proof that it was formed in reality by green and violet rays.

Finally, the yellow spot exhibited similar appearances; altogether invisible through a violet substance, it showed itself green or red through substances of these colours.

Such are the results, that confirm my opinion of the elementary parts of light. Now let these be combined with the effects of absorption, which ultimately leaves only red, green, or violet rays; with the simple and natural explanation of the principal appearances of the spectrum, by means of three kinds of rays; with the happy manner in which these three kinds are applicable to the properties of the dial of colours, and remove its complication; and I think the whole will support my proposition. If it do not hence appear to the natural philosopher as a fact established beyond all question, at least he cannot refuse to consider it as already grounded on strong probabilities, and sufficiently interesting to merit a thorough investigation, which my occupations have prevented me from pursuing any further.

Recapitulation.

Thus our system of colours appears to me reduced to these few data: three sorts of luminous rays, of a particular and unknown nature; red, green, and violet. Combined by twos, the red and green produce yellow; the green

green and violet, blue; the violet and red, purple: the three together produce white; and lastly, the intermediate shades are according to the proportional quantities of their elements.

Bodies exercise a general action on all the rays of light, and a particular one relative to their peculiar nature. If the white pencil fall obliquely on the surface of a transparent body, the rays, as they penetrate it, deviate from their original direction, some more, others less, according to their nature. Here we have a true analysis of white light, in which its three simple elements may be found separate, as well as combined, in different proportions. It is thus that refraction exhibits a series of tints, which differ, in different bodies, both with respect to their general deflection, measured by its mean quantity, in the relative dispersion of the rays, and in the particular position of each colour.

If the affinity of the body for the rays of light, be such as to absorb some into its own substance, it will be coloured; and will exert a preferable or stronger action on certain sorts of rays. In a small mass, the body will first absorb these rays, to which it has a preferable affinity; and, if its action on the two simple kinds do not give a marked preponderance to one of them, it will be a mixed combination, that will first disappear. The mass of the body being gradually increased, the destruction of the rays will go by new mixtures, still progressively; the kind least acted upon will remain the last, and it will necessarily be one of these three, red, green, or violet; after which no more light will be transmitted. Such are the phænomena of absorption, and its different gradations.

I shall give here one of the last results of my experiments, which might have created some confusion had it been mixed with the preceding considerations, yet tends to confirm their principles.

I had an inclination to try whether the light from a given part of the single spectrum, combined with that of another part chosen for the purpose, would produce white.

To

To carry this into execution, I placed before the image of the spectrum, received on the cloth, a screen, by which one portion was concealed, and another left open. This screen, however, was perforated by a small hole, through which passed a ray of coloured light belonging to that part of the spectrum which was concealed. Lastly, this little coloured ray was received on a metallic speculum placed between the screen and the cloth, and inclined so as to throw it on a given point of that part of the spectrum which arrived freely on the cloth. Thus the colour arising from the mixture of two rays of light was observed.

I varied the trial of this apparatus on different points, the corresponding tints of which were diametrically opposite on the dial of colours. In several instances I did not obtain a white free from all tint of colour, because a certain proportion in the quantity as well as quality of the elements is necessary; but having carried the little image almost to the limit of the green and blue, it gave a decided and bright white when thrown on the extremity of the red.

This very remarkable fact adds fresh support to my proposition respecting the compound state of the colour in certain parts of the spectrum, simplified to the utmost. For, if the combination of the three colours I have mentioned be necessary to produce white, as every thing tends to persuade us, we must admit the existence of violet in the greenish blue with which the experiment was made.

N. B. It may be necessary to add, in explanation of fig. 2. Plate IV., that the colours of the three circles are distinguished by dotted lines; the red by round and long points alternately; the green by one long point and two round; the violet by one long and three round.

XXXI. *The*

XXXI. *The Bakerian Lecture, on some Chemical Agencies of Electricity.* *By* Humphry Davy, *Esq. F. R. S. M. R. I. A.*

[Concluded from p. 119.]

VIII. *On the Relations between the electrical Energies of Bodies, and their Chemical Affinities.*

As the chemical attraction between two bodies seems to be destroyed by giving one of them an electrical state different from that which it naturally possesses; that is, by bringing it artificially into a state similar to the other, so it may be increased by exalting its natural energy. Thus, whilst zinc, one of the most oxidable of the metals, is incapable of combining with oxygen when negatively electrified in the circuit, even by a feeble power; silver, one of the least oxidable, easily unites to it when positively electrified; and the same thing might be said of other metals.

Among the substances that combine chemically, all those, the electrical energies of which are well known, exhibit opposite states; thus, copper and zinc, gold and quicksilver, sulphur and the metals, the acid and alkaline substances, afford opposite instances; and supposing perfect freedom of motion in their particles or elementary matter, they ought, according to the principles laid down, to attract each other in consequence of their electrical powers. In the present state of our knowledge, it would be useless to attempt to speculate on the remote cause of the electrical energy, or the reason why different bodies, after being brought into contact, should be found differently electrified; its relation to chemical affinity is, however, sufficiently evident. May it not be identical with it, and an essential property of matter?

The coated glass plates of Beccaria strongly adhere to each other when oppositely charged, and retain their charges on being separated. This fact affords a distinct analogy to the subject; different particles in combining must still be supposed to preserve their peculiar states of energy.

In the present early stage of the investigation, it would be

be improper to place unbounded confidence in this hypothesis; but it seems naturally to arise from the facts, and to coincide with the laws of affinity, so ably developed by modern chemists; and the general application of it may be easily made.

Supposing two bodies, the particles of which are in different electrical states, and those states sufficiently exalted to give them an attractive force superior to the power of aggregation, a combination would take place which would be more or less intense according as the energies were more or less perfectly balanced; and the change of properties would be correspondently proportional.

This would be the simplest case of chemical union. But different substances have different degrees of the same electrical energy in relation to the same body: thus the different acids and alkalies are possessed of different energies with regard to the same metal; sulphuric acid, for instance, is more powerful with lead than muriatic acid, and solution of potash is more active with tin than solution of soda. Such bodies likewise may be in the same state or repellent with regard to each other, as apparently happens in the cases just mentioned; or they may be neutral; or they may be in opposite or attracting states, which last seems to be the condition of sulphur and alkalies that have the same kind of energy with regard to metals.

When two bodies repellent of each other act upon the same body with different degrees of the same electrical attracting energy, the combination would be determined by the degree: and the substance possessing the weakest energy would be repelled; and this principal would afford an expression of the causes of elective affinity, and the decompositions produced in consequence.

Or where the bodies having different degrees of the same energy, with regard to the third body, had likewise different energies with regard to each other, there might be such a balance of attractive and repellent powers as to produce a triple compound; and by the extension of this reasoning, complicated chemical union may be easily explained.

Numerical illustrations of these notions might be made without

without difficulty, and they might be applied to all cases of chemical action; but in the present state of the inquiry, a great extension of this hypothetical part of the subject would be premature.

The general idea will, however, afford an easy explanation of the influence of affinity by the masses of the acting substances, as elucidated by the experiments of M. Berthollet; for the combined effect of many particles possessing a feeble electrical energy, may be conceived equal or even superior to the effect of a few particles possessing a strong electrical energy: and the facts mentioned, page 108, confirm the supposition: for concentrated alkaline lixivia resist the transmission of acids by electricity much more powerfully than weak ones.

Allowing combination to depend upon the balance of the natural electrical energies of bodies, it is easy to conceive that a *measure* may be found of the artificial energies, as to intensity and quantity produced in the common electrical machine, or the Voltaic apparatus, capable of destroying this equilibrium; and such a measure would enable us to make a scale of electrical powers corresponding to degrees of affinity.

In the circuit of the Voltaic apparatus, completed by metallic wires and water, the strength of the opposite electricities diminish from the points of contact of the wires towards the middle point in the water, which is necessarily neutral. In a body of water of considerable length it probably would not be difficult to assign the places in which the different neutral compounds yielded to, or resisted, decomposition. Sulphate of barytes, in all cases that I tried, required immediate contact with the wire: solution of sulphate of potash exhibited no marks of decomposition with the power of 150, when connected in a circuit of water ten inches in length, at four inches from the positive point; but when placed within two inches, its alkali was slowly repelled and its acid attracted*.

Whenever

* In this expriment, the water was contained in a circular glass bason two inches deep, the communication was made by pieces of amianthus of about the

Whenever bodies brought by artificial means into a high state of opposite electricities are made to restore the equilibrium, heat and light are the common consequences. It is perhaps an additional circumstance, in favour of the theory to state, that heat and light are likewise the result of all intense chemical action. And as in certain forms of the Voltaic battery, where large quantities of electricity of low intensity act, heat is produced without light; so in slow combinations there is an increase of temperature without luminous appearance.

The effect of heat, in producing combination, may be easily explained according to these ideas. It not only often gives more freedom of motion to the particles, but in a number of cases it seems to exalt the electrical energies of bodies; glass, the tourmalin, sulphur, all afford familiar instances of this last species of energy.

I heated together an insulated plate of copper and a plate of sulphur, and examined their electricities as their temperature became elevated: these electricities, scarcely sensible at 56° Fahrenheit to the condensing electrometer, became at 100° Fahrenheit capable of affecting the gold leaves without condensation; they increased in a still higher ratio as the sulphur approached towards its point of fusion. At a little above this point, as is well known from the experiments of the Dutch chemists, the two substances rapidly combine, and heat and light are evident.

Similar effects may be conceived to occur in the case of oxygen and hydrogen, which form water, a body apparently neutral in electrical energy to most other substances: and we may reasonably conclude that there is the same exalta-

the eighth of an inch in breadth. The saline solution filled a half ounce measure, and the distance between the solution and the water, at both points of communication, was a quarter of an inch. I mention these circumstances because the quantity of fluid and the extent of surface materially influence the result in trials of this kind. Water included in glass syphons forms a much less perfect conducting chain than when diffused upon the surface of fibrous non-conducting substances of much smaller volume than the diameter of the syphons. I attempted to employ syphons in some of my first experiments; but the very great inferiority of effect as compared with that of amianthus made me altogether relinquish the use of them.

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tion of power, in all cases of combustion. In general, when the different energies are strong and in perfect equilibrium, the combination ought to be quick, the heat and light intense, and the new compound in a neutral state. This would seem to be the case in the instance just quoted; and in the circumstances of the union of the strong alkalies and acids, But where one energy is feeble and the other strong, all the effects must be less vivid; and the compound, instead of being neutral, ought to exhibit the excess of the stronger energy.

This last idea is confirmed by all the experiments which I have been able to make on the energies of the saline compounds with regard to the metals. Nitrate and sulphate of potash, muriate of lime, oxymuriate of potash, though repeatedly touched upon a large surface by plates of copper and zinc, gave no electrical charge to them; subcarbonate of soda and borax, on the contrary, gave a slight negative charge, and alum and superphosphate of lime a feeble positive charge.

Should this principle on further inquiry be found to apply generally, the degree of the electrical energies of bodies, ascertained by means of sensible instruments, will afford new and useful indications of their composition.

IX. *On the Mode of Action on the Pile of Volta, with experimental Elucidations.*

The great tendency of the attraction of the different chemical agents, by the positive and negative surfaces in the Voltaic apparatus, seems to be to restore the electrical equilibrium. In a Voltaic battery, composed of copper, zinc, and solution of muriate of soda, all circulation of the electricity ceases, the equilibrium is restored if copper be brought in contact with the zinc on both sides: and oxygen and acids, which are attracted by the positively electrified zinc, exert similar agencies to the copper, but probably in a slighter degree, and being capable of combination with the metal, they produce a momentary equilibrium only.

The electrical energies of the metals with regard to each other, or the substances dissolved in the water, in the Vol-

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taic and other analogous instruments, seem to be the causes that disturb the equilibrium, and the chemical changes the causes that tend to restore the equilibrium; and the phænomena most probably depend on their joint agency.

In the Voltaic pile of zinc, copper, and solution of muriate of soda, in what has been called its condition of electrical tension, the communicating plates of copper and zinc are in opposite electrical states. And with regard to electricities of such very low intensity, water is an insulating body: every copper-plate consequently produces by induction an increase of positive electricity upon the opposite zinc plate; and every zinc plate an increase of negative electricity on the opposite copper-plate: and the intensity increases with the number, and the quantity with the extent of the series.

When a communication is made between the two extreme points, the opposite electricities tend to annihilate each other; and if the fluid medium could be a substance incapable of decomposition, the equilibrium, there is every reason to believe, would be restored, and the motion of the electricity cease. But solution of muriate of soda being composed of two series of elements possessing opposite electrical energies, the oxygen and the acid are attracted by the zinc, and the hydrogen and the alkali by the copper. The balance of power is momentary only; for solution of zinc is formed, and the hydrogen disengaged. The negative energy of the copper and the positive energy of the zinc are consequently again exerted, enfeebled only by the opposing energy of the soda in contact with the copper, and the process of electromotion continues, as long as the chemical changes are capable of being carried on.

This theory in some measure reconciles the hypothetical principles of the action of the pile adopted by its illustrious inventor, with the opinions concerning the chemical origin of Galvanism, supported by the greater number of the British philosophers, and it is confirmed and strengthened by many facts and experiments.

Thus the Voltaic pile of 20 pairs of plates of copper and zinc exhibits no permanent electromotive power when the connecting

connecting fluid is water free from air *; for this substance does not readily undergo chemical change, and the equilibrium seems to be capable of being permanently restored through it. Concentrated sulphuric acid, which is a much more perfect conductor, is equally inefficient, for it has little action upon zinc, and is itself decomposed only by a very strong power. Piles, containing as their fluid element either pure water or sulphuric acid, will undoubtedly give single shocks, and this effect is connected with the restoration of the equilibrium disturbed by the energies of the metals; but when their extreme plates are connected there is no exhibition, as in usual cases of electromotion. Water containing loosely combined oxygen is more efficient than water containing common air, as it enables oxide of zinc to be formed more rapidly, and in larger quantities. Neutrosaline solutions which are at first very active, lose their energy in proportion as their acid arranges itself on the side of the zinc, and their alkali on that of the copper; and I have found the powers of a combination nearly destroyed from this cause very much revived, merely by agitating the fluids in the cells and mixing their parts together. Diluted acids, which are themselves easily decomposed, or which assist the decomposition of water, are above all other substances powerful; for they dissolve the zinc, and furnish only a gaseous product to the negative surface, which is immediately disengaged.

There are other experiments connected with very striking results, which offer additional reasons for supposing the decomposition of the chemical menstrua essential to the continued electromotion in the pile.

As when an electrical discharge is produced by means of small metallic surfaces in the Voltaic battery, (the opposite states being exalted,) sensible heat is the consequence, it occurred to me, that if the decomposition of the chemical agents was essential to the balance of the opposed electricities, the effect, in a saline solution, of this decomposition,

* The experiments proving this fact, and the other analogous facts in this page, may be seen detailed in Nicholson's Journal, 4to. vol. iv. page 338 and 393; and Phil. Mag. vol. x. page 40.

and

and of the transfer of the alkali to the negative side, and of the acid to the positive side, ought, under favourable circumstances, to be connected with an increase of temperature.

I placed the gold cones, which have been so often mentioned, in the circuit of the battery with the power of 100, I filled them with distilled water, and connected them by a piece of moistened asbestus, about an inch in length and $\frac{1}{6}$ of an inch diameter; I provided a small air-thermometer capable of being immersed in the gold cones, expecting (if any) only a very slight change of temperature; I introduced a drop of solution of sulphate of potash into the positive cone: the decomposition instantly began: potash passed rapidly over into the negative cone, heat was immediately sensible; and in less than two minutes the water was in a state of ebullition.

I tried the same thing with a solution of nitrate of ammonia, and in this instance the heat rose to such an intensity as to evaporate all the water in three or four minutes, with a kind of explosive noise; and at last actual inflammation took place, with the decomposition and dissipation of the greatest part of the salt*.

That the increase of the conducting power of the water by the drop of saline solution had little or nothing to do with the effect, is evident from this circumstance. I introduced a quantity of strong lixivium of potash into the cones, and likewise concentrated sulphuric acid, separately, which are better conductors than solutions of the neutral salts; but there was very little sensible effect.

The same principles will apply to all the varieties of the electrical apparatus, whether containing double or single plates; and if the ideas developed in the preceding sections be correct, one property operating under different modifications is the universal cause of their activity.

* In this process ammonia was rapidly given off from the surface of the negative cone, and nitrous acid from that of the positive cone, and a white vapour was produced by their combination in the atmosphere above the apparatus.

X. *On some general Illustrations and Applications of the foregoing Facts and Principles, and Conclusion.*

The general ideas advanced in the preceding pages are evidently directly in contradiction to the opinion advanced by Fabroni, and which, in the early stage of the investigation, appeared extremely probable, namely, that chemical changes are the primary causes of the phænomena of Galvanism.

Before the experiments of M. Volta on the electricity excited by the mere contact of metals were published, I had to a certain extent adopted this opinion; but the new facts immediately proved that another power must necessarily be concerned; for it was not possible to refer the electricity exhibited by the apposition of metallic surfaces to any chemical alterations, particularly as the effect is more distinct in a dry atmosphere, in which even the most oxidable metals do not change, than in a moist one, in which many metals undergo chemical alteration.

Other facts likewise soon occurred demonstrative of the same thing. In the Voltaic combination of diluted nitrous acid, zinc and copper, as is well known, the side of the zinc exposed to the acid is positive. But in combinations of zinc, water and diluted nitric acid, the surface exposed to the acid is negative; though if the chemical action of the acid on the zinc had been the cause of the effect, it ought to be the same in both cases.

In mere cases of chemical change likewise electricity is never exhibited. Iron burnt in oxygen gas, properly connected with a condensing electrometer, gives no charge to it during the process. Nitre and charcoal deflagrated in communication with the same instrument do not by their agencies in the slightest degree affect the gold leaves. Solid pure potash and sulphuric acid made to combine in an insulated platina crucible produce no electrical appearances. A solid amalgam of bismuth and a solid amalgam of lead become fluid when mixed together: the experiment, I find, is connected with a diminution of temperature, but with no exhibition of electrical effects. A thin plate of zinc, after being placed

placed upon a surface of mercury, and separated by an insulating body, is found positive, the mercury is negative: the effects are exalted by heating the metals; but let them be kept in contact sufficiently long to amalgamate, and the compound gives no signs of electricity. I could mention a great number of other instances of pure chemical action in which I have used all the means in my power to ascertain the fact, and the result has been constantly the same. In cases of effervescence, indeed, particularly when accompanied by much heat, the metallic vessels employed become negative, but this is a phænomenon connected with evaporation, the change of state of a body independent of chemical change, and is to be referred to a different law*.

I mentioned the glass plates of Beccaria as affording a parallel to the case of combination in consequence of the different electrical states of bodies. In Guyton de Morveau's experiments on cohesion, the differerent metals are said to have adhered to mercury with a force proportional to their chemical affinities. But the other metals have different electrical energies, or different degrees of the same electrical energy with regard to this body; and in all cases of contact of mercury with another metal, upon a large surface, they ought to adhere in consequence of the difference of their electrical states, and that with a force proportional to the exaltation of those states. Iron, which M. Guyton found slightly adhesive, I find exhibits little positive electricity

* The change of the capacities of bodies in consequence of the alteration in their volumes or states of existence by heat, is a continually operating source of electrical effects; and, as I have hinted page 117, it often interferes with the results of experiments on the electrical energies of bodies as exhibited by contact. It is likewise probably one of the sources of the capricious results of experiments of friction, in which the same body, according as its texture is altered, or its temperature changed, assumes different states with regard to another body. Friction may be considered as a succession of contacts, and the natural energies of bodies would probably be accurately exhibited by it, if the unequal excitation of heat or its unequal communication to the different surfaces did not interfere by altering unequally their electrical capacities. Of the elements of flint glass, silex is slightly negative with regard to the metals, the soda is positive; and in contacts of glass with metals I find it exhibits the excess of the energy of the alkali: the case, as is well known, is the same in friction, the amalgam of the common machine is essential to its powerful excitation.

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after being laid upon a surface of mercury, and then separated. Tin, zinc, and copper, which adhere much more strongly, communicate higher charges to the condensing electrometer. I have had no instrument sufficiently exact to measure the differences; but it would seem that the adhesion from the difference of electrical states must have operated in these experiments*, which being proportional to the electrical energies are, on the hypothesis before stated, proportional to the chemical affinities. How far cohesion in general may be influenced or occasioned by this effect of the difference of the electrical energies of bodies, is a curious question for investigation.

Many applications of the general facts and principles to the processes of chemistry, both in art and in nature, will readily suggest themselves to the philosophical inquirer.

They offer very easy methods of separating acid and alkaline matter, when they exist in combination, either together or separately, in minerals; and the electrical powers of decomposition may be easily employed in animal and vegetable analysis.

A piece of muscular fibre, of two inches long and half an inch in diameter, after being electrified by the power of 150 for five days, became perfectly dry and hard, and left on incineration no saline matter. Potash, soda, ammonia, lime, and oxide of iron were evolved from it on the negative side, and the three common mineral acids and the phosphoric acid were given out on the positive side.

A laurel leaf treated in the same manner, appeared as if it had been exposed to a heat of 500° or 600° Fahrenheit, and was brown and parched. Green colouring matter, with resin, alkali, and lime, appeared in the negative vessel: and the positive vessel contained a clear fluid, which had the smell of peach blossoms; and which, when neutralized by potash, gave a blue-green precipitate to solution of sulphate of iron; so that it contained vegetable prussic acid.

A small plant of mint, in a state of healthy vegetation,

* Amalgamation undoubtedly must have interfered; but the general result seems to have been distinct.

was

was made the medium of connection in the battery, its extremities being in contact with pure water; the process was carried on for 10 minutes: potash and lime were found in the negatively electrified water, and acid matter in the positively electrified water, which occasioned a precipitate in solutions of muriate of barytes, nitrate of silver, and muriate of lime. This plant recovered after the process: but a similar one, that had been electrified for four hours with like results, faded and died*. The facts show that the electrical powers of decomposition act even upon living vegetable matter; and there are some phænomena which seem to prove that they operate likewise upon living animal systems. When the fingers, after having been carefully washed with pure water, are brought in contact with this fluid in the positive part of the circuit, acid matter is rapidly developed, having the characters of a mixture of muriatic, phosphoric, and sulphuric acids: and if a similar trial be made in the negative part, fixed alkaline matter is as quickly exhibited.

The acid and alkaline tastes produced upon the tongue, in Galvanic experiments, seem to depend upon the decomposition of the saline matter contained in the living animal substance, and perhaps in the saliva.

As acid and alkaline substances are capable of being separated from their combinations in living systems by electrical powers, there is every reason to believe that by converse methods they may be likewise introduced into the animal œconomy, or made to pass through the animal organs; and the same thing may be supposed of metallic oxides; and these ideas ought to lead to some new investigations in medicine and physiology.

It is not improbable that the electrical decomposition of

* Seeds, I find, when placed in pure water in the positive part of the circuit, germinate much more rapidly than under common circumstances; but in the negative part of the circuit they do not germinate at all. Without supposing any peculiar effects from the different electricities which however may operate, the phænomenon may be accounted for from the saturation of the water near the positive metallic surface with oxygen, and of that near the negative surface with hydrogen.

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the neutral salts in different cases may admit of œconomical uses. Well burned charcoal and plumbago, or charcoal and iron, might be made the exciting powers; and such an arrangement if erected upon an extensive scale, neutrosaline matter being employed in every series, would, there is every reason to believe, produce large quantities of acids and alkalies with very little trouble or expense.

Ammonia and acids capable of decomposition, undergo chemical change in the Voltaic circuit only when they are in very concentrated solution, and in other cases are merely carried to their particular points of rest. This fact may induce us to hope that the new mode of analysis may lead us to the discovery of the true elements of bodies, if the materials acted on be employed in a certain state of concentration, and the electricity be sufficiently exalted. For if chemical union be of the nature which I have ventured to suppose, however strong the natural electrical energies of the elements of bodies may be, yet there is every probability of a limit to their strength: whereas the powers of our artificial instruments seem capable of indefinite increase.

Alterations of electrical equilibrium are continually taking place in nature; and it is probable that this influence, in its faculties of decomposition and transference, considerably interferes with the chemical alterations occurring in different parts of our system.

The electrical appearances which precede earthquakes and volcanic eruptions, and which have been described by the greater number of observers of these awful events, admit of very easy explanation on the principles that have been stated.

Besides the cases of sudden and violent change, there must be constant and tranquil alterations in which electricity is concerned, produced in various parts of the interior strata of our globe.

Where pyritous strata and strata of coal-blende occur, where the pure metals or the sulphurets are found in contact with each other, or any conducting substances, and where different strata contain different saline menstrua, electricity must be continually manifested; and it is very probable, that many

many mineral formations have been materially influenced, or even occasioned by its agencies.

In an experiment that I made of electrifying a mixed solution of muriates of iron, of copper, of tin, and of cobalt, in a positive vessel, distilled water being in a negative vessel, all the four oxides passed along the asbestus, and into the negative tube, and a yellow metallic crust formed on the wire, and the oxides arranged themselves in a mixed state round the base of it.

In another experiment, in which carbonate of copper was diffused through water in a state of minute division, and a negative wire placed in a small perforated cube of zeolite in the water, green crystals collected round the cube; the particles not being capable of penetrating it.

By a multiplication of such instances the electrical power of transference may be easily conceived to apply to the explanation of some of the principal and most mysterious facts in geology.

And by imagining a scale of feeble powers, it would be easy to account for the association of the insoluble metallic and earthy compounds, containing acids.

Natural electricity has hitherto been little investigated, except in the case of its evident and powerful concentration in the atmosphere.

Its slow and silent operations in every part of the surface will probably be found more immediately and importantly connected with the order and œconomy of nature; and investigations on this subject can hardly fail to enlighten our philosophical systems of the earth; and may possibly place new powers within our reach.

Explanation of the Figures. Plate III.

Fig. 1, Represents the agate cups, mentioned page 5.

Fig. 2, Represents the gold cones, page 7.

Fig. 3, Represents the glass tubes, and their attached apparatus, page 105.

Fig. 4, Represents the two glass tubes, with the intermediate vessel, page 106.

In all the figures AB denote the wires, rendered one positively, the other negatively electrical; and C the connecting pieces of moistened amianthus.

XXXII. *His-*

XXXII. *History of Astronomy for the Year* 1806. *By* JEROME DE LALANDE.

[Concluded from p. 129.]

THE *metre des archives* of the legislative body and hat of the observatory have been placed by the new comparator of M. Le Noir at 23° of the centigrade thermometer: they differ only by a 600,000dth part from each other, but we cannot account for this difference: the comparison was made by Messrs. Delambre, Prony, Burckhardt, and Bouvard.

M. Chevalier, keeper of the library at the Pantheon, who accompanied Mechain into Spain, has re-established the observatory of Pingrè with very excellent instruments.

M. Leupold, who has laboured along with me in Paris, has undertaken to re-establish the observatory at Bourdeaux; he is assisted by Messrs. Lescar and Ducum. They have applied to the administration to enable them to proceed with the necessary repairs; and they propose to procure a quarter of a circle. This establishment will certainly be of great use in a place where there are such a number of seamen.

The marine department has furnished some works this year. In the third volume, for 1806, of the Ephemerides of Coimbra, there is a table of longitudes made from the triangles of Portugal. The Hydrographic Board at Madrid has furnished several of them. We there find a memoir upon the rhomboidal reticule, and one upon the use of the meridian glass, when it has a deviation. We there find *Demonstraçao e ampliaçao do Calculo dos Eclipses proposto no primeiro Volume das Ephemerides de Coimbra*, 1806.

But this method does not seem to me to have any advantage over the others; it proceeds upon the tables of the Ephemerides of Coimbra.

Elémens Historiques-pratiques de la Marine, by M. Suzanne. We there find the calculation of the resistance of forces, with naval tactics and manœuvres.

M. Ducum, professor of navigation at Bourdeaux, has published a memoir entitled "New Method for determining the

the Latitude at Sea by Altitudes taken out of the Meridian, and the Longitude by an Altitude of the Moon in several Cases." 36 pages 4to. He shows the inconveniences into which we may fall by employing the method of d'Awes, and the precautions which may be taken to avoid them. He was not acquainted with the grand work with which M. Mendoza was occupied.

With respect to the method of the longitudes, it is long since Lemonnier, Pingrè, and myself, showed its utility; it is specially indicated in page 68 of my *Abregé de Navigation*, as being able to simplify considerably the method of the longitudes by means of the horary tables I have published with this abridgment.

The new lunar tables of M. Burg serve for calculating the observations of longitudes made in the voyage of New Holland; and Michael Lalande has calculated for this purpose several observations of the moon at the meridian.

It remains to recalculate all the observations made in the three voyages of Cook, because they have been compared with the lunar tables we had at that time, and which were not exact enough to hold the place of corresponding observations.

Geography has this year made new progress. Mungo Park has written from Tombuctoo on the coast of Africa. A report had been spread of his death. His return will make us acquainted with some curious circumstances respecting these unknown countries.

The vessels the Navesda and the Neva, which left Cronstadt on the 7th of August 1803, under the orders of captain Krusenstern, returned on the 4th and 19th of August 1806, after having sailed round the world. This is the first circumnavigation of the Russians. M. Horner made several important observations for geography upon the coast of Tartary. There are several of them in the Journal of Gotha for September 1806, in which baron Zach continues to publish every thing interesting in astronomy; another proof how necessary it is for an astronomer to be acquainted with German.

M. Nisnieuski, astronomer to the Petersburgh Academy, has

has made some journeys and voyages for determining the principal points of the new acquisitions to the Russian government in Lithuania, Volhynia, Podolia, and Taurida.

In the spring of 1804, captain Lewis was directed by the president of the United States to ascend the Missouri to discover the easiest way of reaching the Pacific ocean *. For this purpose he left Washington, accompanied by captain Clark and thirty men, forming a small caravan: they ascended the Missouri to within 930 leagues of its grand cataracts; they then traversed the rocky mountains in the neighbourhood, where they were under the necessity of wintering, on account of the snow with which these mountains were covered; and in one part, extending about twenty leagues, the snow never melts. Captain Lewis proceeded 120 leagues to the navigable part of the river Kooskooske; from Kooskooske to the south-east branch of the river Columbia, 25 leagues; from this branch to the bed of the same river, 50 leagues; and, lastly, they travelled along the river Columbia to its mouth, being 150 leagues: which gives a distance of more than 1200 leagues from the mouth of the Missouri to the Pacific ocean.

Captain Lewis observed that the tide flowed 30 leagues up the Columbia; that is to say, to within two leagues and a half of its cataracts, to the place where it is navigable for large vessels. Above this place it is only navigable for canoes and flat-bottomed boats.

The *Rélation du Voyage de Découvertes faites aux Terres Australes pendant les Années* 1800, 1801, 1802, 1803, 1804; comprising, 1st, The historical part: 2d, The manners and description of the people: 3d, The physical and meteorological part; forming together four vols. in 4to, edited by Messrs. Peron and Lesueur, will be published at the expense of government.

The natural history part of the same voyages and travels will be printed and published by subscription.

M. Gualtie, a naval officer belonging to St. Maloes, has made a great number of observations in the country of the Mississippi, in North America.

* See Phil. Mag. vol. xxvii. p. 13, &c.

M. de

M. de Lowenhohn has published in Denmark a chart and description of the Faro isles between latitude 61° 25′ and 62° 25′, in order to form a continuation to the grand and beautiful chart of Denmark, in several sheets, which has been drawn by the Danish geographical engineers.

The proposed visit of the missionaries to China not having been accomplished this year, the instruments intended for these travellers have been in the mean time deposited in the observatory.

M. Buache has given notice of a curious chart for the history of geography,—*La Mappamonda di fra Mauro descrito ed illustrato da placido zurla Venezia* 1806, in folio. This map of the world, made at Venice about the year 1457, is curious, as it makes us acquainted with the geography of that period: it was drawn for Alphonsus V., king of Portugal.

M. Dupuis, author of the grand work *l'Origine des Cultes*, continues his researches upon the same subject. He has discussed thirty antient theogonies; he has compared the constellations of China and of the Indies; and he has made a curious planisphere of them. Among the singularities which his inquiries have presented to him, we may reckon the explanation of the four rivers of the terrestrial Paradise, which are the expressions of the four signs of the zodiac. According to Joseph, the first expresses *plenitude*, being the part of long days. The second, being *dispersion*, answers to the autumnal equinox. The third, being narrow and rapid, expresses the short days of winter. The fourth signifies the *coming from the east*; this implies the rising of the sun in the regeneration of spring by the reforming Lamb, which effaces the evils of winter at his heliac rising,—an emblem of Christ, who wipes away the sins of the world. The Chinese have also the *Yellow river*, the *Red water*, and the *Water of the Lamb*, according to lord Macartney.

M. Dupuis has also published a "Memoir for explaining the chronological and mythological Zodiac;" a work containing the picture of the houses of the Moon among the various nations of the East, and a view of the most antient observa-

observations connected with it, according to the Egyptians, the Chinese, the Persians, the Arabians, the Chaldeans, and the Greeks.

This zodiac, which has been engraved in a very superior manner at the expense of M. Volney, contains the 27 natchtrons of the Indians, the 28 under the Chinese, the 28 kordens of the Persians; that is to say, the houses which the moon passes through in a month: and the learned author has drawn from these comparisons results which are extremely curious, in-so-far as the antiquity of these various zodiacs is concerned; among others this important idea, that we ought not to confine ourselves to the observations which have been transmitted to us; the initial point of the division being more antient, while we have only been able to date it from the equinoxes and solstices 4700 years before the vulgar æra.

This gives him the explanation of the Indian fables of the Phœnix, which is only the period of 1460 years, and several other curious points of erudition. He is at present occupied with his collection of thirty theogonies, in order to complete his great work on the origin of the different religions. He remarks, for example, that Adam and Eve are the constellations of Charles's wain and of Virgo, each of which has a serpent, and which are near the autumnal equinox. When Adam rises, Eve seems to follow him, and she announces the entrance of evil into the world.

There has been printed in the 47th volume of the Academy of Inscriptions, a long memoir of M. de Guignes on the origin of the zodiac of the orientalists, extracted from a distinct work concerning the Egyptians and Chinese, not yet printed. He thinks that the Greeks, for want of being better acquainted with what the Egyptians taught as to the course of nature, formed a zodiac; but the names of the Ram, the Bull, &c. are not names of constellations; it is a division of the year into twelve parts, corresponding to the productions of the earth and the influence of the sun. We see throughout the whole of the east a crowd of constellations, which take up the places of the Ram, &c.: these

signs,

signs, therefore, did not exist antiently. M. de Guignes infers from what we now know of the Chinese system, that the Egyptian was the same.

The losses to science this year from the deaths of learned men have not fallen upon astronomers; but M. Coulomb, to whom magnetism lies under great obligations, deserves our regret, as I have shown in the discourse I pronounced over his tomb, and which I printed: M. Delambre has also noticed his death in an elegant manner in the Memoirs of the Institute.

In the Ephemerides of Milan for 1806, M. Cesaris has given the Life of Francis Reggio, born at Genoa on the 25th of April 1743, and who died at Milan on the 12th of October 1804, whose labours have illustrated the Milan observatory for these forty years. I have often taken occasion to speak of the Ephemerides of Milan. M. Reggio has been replaced by M. Carlini: M. Flaugergues has published the eulogy of M. de Ratte; M. Vitalis has read to the Academy of Rouen that of M. Dulague.

I forgot to mention in the History of Astronomy for 1804, George Shuckburgh, of whom I have spoken in my Travels through Italy, on account of the altitudes of mountains measured by him in several countries (Phil. Trans. 1777); and also on account of the fine equatorial he caused to be constructed upon his estate, (*Astronomie*, art. 2410.) It is a great loss to the science that this instrument must be now useless for want of an observer.

They write from Ratisbon, of the 5th of March, that count Sternberg, vice-president of the electoral directory, has published a project of a subscription for erecting a colossal statue of Carrara marble to the celebrated astronomer Kepler, who died at Ratisbon in 1630, on his way to Vienna. The elector was the first on the list of subscribers for the sum of 1000 florins.

Marshal Ney having entered Thorn on the 6th of December, I wrote to request him to erect a small monument to the great Copernicus, who was born in this city on the 19th of February 1473: this monument will preserve the memory

memory of the conquests of the French, and their taste for the sciences.

The meteorology of this year has presented some extraordinary variations; there have been neither any very great heats nor colds, but plenty of tempests.

In November 1805 there was an earthquake at Frankfort. On the 20th of January 1806 there was a rumbling noise at Orgon and the mouths of the Rhone, which awakened the inhabitants. On the 10th of January there were storms at Bourdeaux, Brest, Rouen, Paris, and Ypres.

On the 11th of January there was a dreadful storm in the neighbourhood of Chartres, Dijon, and Nancy; an extraordinary hurricane at Besançon; there was a thunder storm at Toulouse, and there was an inundation at Rotterdam.

On the 20th, in the department of Landes, there were some buildings burnt, and some trees torn up by the roots.

On the 28th there was experienced at Poitiers two shocks of an earthquake, accompanied by a prolonged rumbling noise; the thunder fell upon the steeple of Cervo, near Savone, and the steeple of Albisola was thrown down; it fell upon the steeple of Bezaudun, near Grassè. Thus these stormy clouds occupied more than a hundred leagues in extent; which confirms the explanation of hurricanes which I gave in the History of Astronomy for 1805.

On the 13th of March, at Havre, Caen, Antwerp, and Brest, there were dreadful hurricanes; and several shipwrecks on the coast of Holland.

On the 9th of June there was a dreadful hurricane at Salerno.

M. Blanc, captain of a corps at Palma Nova, in Venetian Frioul, observed a singular water-spout, which I gave an account of in the *Journal de l'Empire* of the 22d of August, and in the *Moniteur* of the 23d.

On the 29th there was a very strong wind at Nantes: at Paris some chimneys were thrown down, trees were torn up by the roots, and all the fruits were shaken off.

In September there was a dreadful storm at Martinique: 300 persons perished at Dominica.

On

On the 8th of August there was an earthquake upon the banks of the Jenisseick, in Siberia.

On the 11th of December, in the neighbourhood of Mayenne and Vitrie, three steeples were destroyed by thunder: on the 13th it thundered at Brest: on the 14th it was as fine a day at Paris as any in spring; the cold commenced after the moon's passage through the equator.

M. Lamarck has published, for the eighth time, his Meteorological Annuary for 1807: he congratulates himself for having had the courage to devote himself to the study, and for undertaking the publication of this annuary: at first he only considered the two alternate declinations of the moon; at present he considers the different systems of the moon's influence, and the perpetual substitution of the one for the other. These different systems change incessantly their relations and their effects at every lunar point; the new moon, apogée, equinoxes, and solstices, vary on account of the general disposition of the other points at the epoch at which they arrive; and he deduces from this, for every month, probabilities for the temperature.

He has given in the 7th volume, for 1806, an epitome of his meteorological system; which is only applicable to the generalities of the atmosphere. He gives in the latter the influences which the general disposition of the lunar points seems to him to exercise upon every arriving point: whence result for this point modifications which render its action sometimes efficacious, and sometimes incapable of being so.

He has added some memoirs upon easterly and westerly winds, and upon the difference of phænomena, upon atmospheric electricity, and upon the courses of its variations.

I am desirous that observers should examine and verify the principles and results, which have been perhaps too much neglected hitherto. If there were a great number of points of observation, we might succeed in following the direction of a wind from its origin, and thereby arrive at its cause, and follow these variations and effects.

After having announced all the good books which have appeared,

appeared, in order that we may read them, it may be useful to speak of those whose titles might deceive us.

Essai sur le Monde, by M. Azais, 1806, in 8vo. He endeavours to prove the necessity of a first impulsion; the infinity of the universe and light as the cause of movement by the impulsion of rays, and as the cause of gravitation, the impulsion of the stars being greater than that of the sun: he read one part of these singular ideas to the Institute. The *Mouvement de la Matiere en tout Sens*, and the *Corpuscules ultramondains*, with which M. Lesage has been busy all his life at Geneva, may serve as a support to the system of M. Azais: but scientific men disdain these kinds of hypotheses, as being destitute of proof, and not being capable of teaching us any thing.

M. de Lormel has published supplements to his *Grande Periode*, in order to give new proofs and new applications for the six days of the creation and the prophecies of Daniel. He endeavours to refute what I have already objected to him, that the diminution of the obliquity of the ecliptic cannot form a period, since it cannot continue according to the well-known cause of this diminution.

There has appeared a book, the title of which deceived me, because I never thought of meeting with any thing like astronomy in it. It is entitled *Hommage à Pius* VII. *et Napoleon-le-Grand*, or *Junction of Religions*; by Alex. Jos. Guyot, curate of Cambray. He speaks of the atmosphere of the comets; he thinks he has discovered the size of the stars: but it may be easily seen that he confounds the apparent diameter of the stars with the annual parallax, which can enable us to judge of their distance. He is persuaded that he has demonstrated that the sun does not turn round, because the moon would each day have all its phases. He does not comprehend that, the moon having a semi-diurnal motion with the sun, it is all one whether this movement be apparent or real.

Thus, all these astronomical articles prove that he has not profited by my lectures, at which he says he was present.

"Discovery of the Orbit of the Earth; of the central Point;

Point; of the Orbit of the Sun; their Situation and Forms of the Section of the Zodiac by the Plane of the Equator; concordant Movement of the two Globes: by M. C. I. E. d'Aguila, 1806."

The author first explains all the known systems; he afterwards gives an astrostatic, in which he asserts that the sun traverses the ecliptic with a very large circle, and that the earth traverses the equator with a very small circle. He rejects all eccentricity, as being contrary to the divine wisdom of the Creator; he is so riveted to these absurdities, that in page 256 he says, when speaking of the mean sun which I made use of for the equation of time: "There is no occasion for quoting such an author, but we should turn to the sacred writings, where every thing is published, and which has tended more than any other writing to form authors for this half century past; whilst the works of this writer have been so much filled with the absurdities and errors of modern philosophy, that posterity will only preserve some leaves of them; that is to say, the notions furnished by artisans for the part of the measures."

In speaking of the refraction, which shows a star at the horizon although it is still below it, he adds: "These burlesque metaphysics, which would be laughed at by the savages of California, have been supported by the common-place tables furnished according to a false principle of an unique centre." He does not even understand that there are six months of constant night under the pole; he finishes by saying,—"May this publication participate in the enlightening of the nineteenth century!"

"*Nouvelle Découverte:* being one of the grandest Discoveries that Men have ever yet made, comprehending the natural and physical Cause of that beautiful Phænomenon in the Sky called the *Milky Way:* by Laurent Potier des Laurieres." I should not mention this pamphlet if it had not been announced as having been presented to the legislative body. The author thinks that the rays of the sun, broken and divided by the contact of the earth, go to form the circle of the *milky way,* which is only 180 leagues distant, and which

runs through the whole circle of our horizon in the space of 29 years.

There is also published a pamphlet entitled "New Discovery of the Flux and Reflux of the Sea," the physical cause of which is said to be the double movement of the globe. A chimera of the Newtonian attraction. This may suffice to show the absurdity of the work.

The same author has also published a new discovery of the quadrature of the circle, which shows that nothing can be expected from his astronomy.

There is likewise a book published called "Elements of the Philosophy of the Heavens." I was very much deceived by this title: I thought it was a book upon astronomy, but it is upon devotional exercises.

XXXIII. *On the Alkalies of Commerce, and on the least expensive Process for ascertaining their commercial Value by Means of the Instrument called the Alkali-meter. Read to the Academy of Rouen, 5 Thermidor, An. 13, by M.* Descroizilles *senior.*

[Continued from p. 178.]

Invariability and Facility of the Alkali-metrical Proofs.

A graduation like that which we have been describing is truly natural and invariable; there is nothing precarious in it: in short, let us examine the operation we have described.

We have, on the one hand, put into solution one decagramme, that is to say, ten grammes or 100 decigrammes of potash; nevertheless we have only taken definitively the moiety of this solution; thus it is reduced to 100 half-tenths of the grammes: on the other hand, we have put into the alkali-meter 38 grammes, that is to say, 76 demi-grammes of a liquor composed of one part in weight of sulphuric acid and of nine similar parts of water: the 76 demi-grammes of the test liquor therefore contain 76 demi-tenths of grammes of effective acid; and, if it required 55 demi-grammes of this liquor

liquor in order to saturate the proved potash, we are evidently well founded in saying that this alkali has absorbed a quantity of sulphuric acid equal to 55 hundredths of its weight.

Each trial, however, is made without calculation; a single weight is sufficient, and the instrument finishes the rest almost alone.

In some countries from which potash is exported there are inspectors of this article; but they have not had, until this moment, any method of inspection which could be compared with that described in this memoir *.

Alkali-metrical Trials of Sodas, Natron, Tartar Ashes, Tobacco Ashes, &c.

It requires but a very slight modification in this process to apply it to the trial of sodas; in place of one decigramme, *i. e.* ten grammes, we must put about ten and a half, or, if we please, three drachms, in a middle-sized metal mortar, and pound them strongly for five minutes, so as to produce a fine powder, of which we must weigh exactly one decigramme, or, what is very near that quantity, two drachms and forty-four grains and two-thirds of a grain, which we afterwards put in the water, as is said above of potash.

Sicilian ashes, or the ashes of the rocket plant, and other similar alkaline substances, are tried like sodas.

Natron, tartar ashes, tobacco ashes, &c. also require to be bruised before weighing the ten grammes for the trial.

Ordinary Alkali-metrical Results.

The various alkaline substances present the alkali-metrical degrees marked in the following table: these marks are the results of several thousand trials, which I have had occasion to make for these five-and-twenty years.

* I might here discuss some appearances of uncertainty observed at the time of my alkali-metrical trials, upon some potashes and sodas, containing both sulphurets and sulphites; but they do not prevent me from asserting, that, in general, the alkali-meter I now propose points out sufficiently for the chemical arts the comparative value of the alkalies of commerce.—*Note by the Author.*

American pearl-ashes; best sort	-	60° to 63°*
American caustic potash in reddish masses; first sort	- - - -	60 — 63
American pearl-ashes; second sort		50 — 55
Caustic potash of America, in grayish masses; second sort	- -	50 — 55
White potash of Russia	- -	52 — 58
White potash of Dantzic	- -	45 — 52
Blue Dantzic potash	- -	45 — 52
Alicant soda	- - -	20 — 33 *
Natron	- - - -	20 — 33
Soda and natron of inferior qualities		10 — 15

Extraordinary Alkali-metrical Results.

Great variations are met with in some parts of these articles; in the pearlashes and potashes, for instance, by the fraudulent mixture of various foreign salts, the alkaline part of which, neutralized by the sulphuric and muriatic acids, is of no use to soap-makers, dyers, and bleachers: we meet with variations also in the sodas by means of the addition of herbs, other than the salsola and the salicornia at the time of incineration; or also in sodas produced by the combustion of plants before their maturity. For, according to M. Chaptal, the latter is almost entirely muriate without alteration. Certain potashes contain also sulphur and charcoal in solution: the casks and packages of these alkalies are not always of the same quality in their different points and various pieces. The two extremities of a cask, and its centre towards the bung, have been found to contain good potash: the other parts were of a very inferior quality, or contained nothing else than earth. Some unprincipled people have made a traffic of soda from sea wrack, which they sell in place of Alicant soda, giving it an exterior appearance by means of a process, which it would be useless if not criminal to divulge.

* Salt of soda of M. Carny.

No. 1.	- - -	70
No. 2.	- - -	46
No. 3.	- - -	36.—*Note by the Author.*

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On the other hand, I have in my experiments sometimes met with extraordinary degrees in point of strength, such as pearlashes at 66°; American potashes of the first sort at 72°; and even these potashes, marked second sort, at 66°, and, lastly, pieces of natron at 60°. These last had probably been deprived of all water of crystallization.

Great Utility of a Method of Graduation for the Alkalies of Commerce.

It results from the preceding observations, that not only the consumer of the alkalies of commerce has hitherto only had very precarious methods of estimating comparatively the real pecuniary value of what he buys, but also that he is exposed to very troublesome inequalities in the result of his operations, if he employs, as almost always happens, the good and the bad commodities in equal doses. Whether a dyer, for instance, employs soda at 30° or at 15°, he will be equally exposed to fail in his operations, and consequently to experience great losses, either from his alkali being weaker or stronger than usual.

On extending these reflections, we see that all the arts which require the consumption of alkalies would consequently receive a great benefit from the happy innovation which would be produced in this branch of commerce, if, on the one hand, potashes, sodas, and natron, should become constantly homogeneous in the same cask, which would bear a mark indicating its alkaline strength; and if, on the other hand, we could announce in the prices current that such an alkali, of a certain alkali-metrical degree, costs so much the cwt. It will be very difficult, however, to obtain these happy effects; they will be produced by the spirit of the first trader who shall see the good effects of it. We may conceive, in fact, what advantage a dealer in this article would have over his neighbours, who could say to his customers,—" I offer you graduated potash, graduated soda, graduated salt of soda, and graduated natron, at such and such prices and alkali-metrical degrees. You have now in your possession an instrument with which you may, without chemical knowledge and without calculations, try or cause

to be tried under your own inspection one hundred specimens in a day, and thus verify the qualities I announce to you in my prices current, and which are marked upon my casks and packages." It cannot be doubted that the merchant who could deal in this manner would obtain a great preference, and ultimately others would be induced to pursue the same course. I shall now proceed with my detail of the method of accomplishing this process, beginning with the

Graduation of Potashes.

Suppose there are in a storehouse 100 casks of potash, containing about 400 kilogrammes each: we may give all of them the same alkaline strength, and may afterwards ascertain it, and point it out in a precise manner.

Each of the casks is opened at one end, and the potash it contains is emptied upon the ground. We must afterwards break, with a long-handled mallet, all the masses which are larger than a large nut; the whole must then be mixed equally, which may be done in five minutes by two men furnished with wooden shovels. The potash must be afterwards replaced in the casks, arranging the business so that the casks, whether full or not, shall contain the same number of pailfuls or measures. All these casks having been thus opened, their contents broken and mixed up, a common mixture may be once more made of all their contents.

For this purpose, a place being set apart on the floor of the store-room, we must bring, one after another, a pailful of the potash from each barrel alternately; a sufficient number of men being furnished with wooden shovels, the whole mass may be mixed up as exactly as possible. The casks are afterwards filled; and by this means we may ascertain that their contents have an equal degree of alkaline strength, which may be proved by a single trial.

This trial must be done with exactitude, and in the following manner: we must put about two kilogrammes of the general mixture into a mortar, and pound them until no piece remains larger than a grain of wheat. One decigramme must then be weighed exactly, which, upon being subjected to the proof by the alkali-meter, will indicate the alkaline strength of the whole potash in the casks.

It

It is easy to prevent any loss of weight in any of the casks, by care and celerity in the manipulation; during which, in damp seasons, a stove should be placed in the store-room: if this precaution is not adopted, there will be an increase of weight.

Graduation of Sodas and Natron.

It will not be difficult to apply these manipulations to soda: this article must be first broken by means of a piece of iron, then pounded in mortars, or bruised under a vertical mill the stamper of which is inserted in a wooden axis: the soda in powder must then be passed through a metallic sieve for the convenience of those who use it. As soon as the quantity is procured requisite to fill a given number of casks, we must then proceed to make the general mixture above described, and pursue the usual method, with the peculiar modifications applicable to the trials upon soda.

As to natron, the same method may be adapted as with potash, with this exception, that, the masses being generally larger and harder, they require a little more time and manipulation than potash.

Œconomy of the Graduation of the Alkalies of Commerce.

Some people may be apprehensive that the expense of these manipulations would counterbalance the advantages to be derived from them; but, after having made a gross calculation of the expenses of this manipulation upon potash and natron, I found they were very trifling: as to the expenses upon the soda, these will be reduced one-half if we deduct the expense of pulverization, which is, at all events, an indispensable requisite with this article.

Property possessed by the Alkali-meter of being used as a Bertholli-meter.

I have the honour to transmit herewith to the academy a tube at one and the same time alkali-metrical and Bertholli-metrical. The same scale serves for both, with this difference, that the alkali-metrical scale is on the right side and runs from top to bottom, but the Bertholli-meter is on the

the left side, and runs from bottom to top. The engraving (Plate IV.) will explain this two-fold arrangement. The Bertholli-meter by this means is much simpler than it was when I gave the first description of it. I should, indeed, mention that the new Bertholli-meter has not the same precision as the first. As it is, however, it cannot fail to be very useful to bleachers after the Berthollian process. I take pleasure in rendering this new homage to a chemist so celebrated, and to whom we owe an art from which commerce in general, and that of Rouen in particular, has derived immense advantages*.

Constant Proportions of Carbonic Acid in the Salt with Excess of Base, which is extracted from all the Ashes.

Of all the acids combined with the alkalies the carbonic acid is that which deprives them the least of their energy, and which may be taken from them with most facility; it is by means of lime that, from time immemorial, the effect has been produced upon which several chemical arts have been founded, and particularly that of the soap-makers; but, as the existence of carbonic acid was scarcely known formerly, the art of combining it with lime, by taking it from the alkalies, is felt even at the present time, as a consequence of the antient imperfection of chemical knowledge. Although for more than five-and-twenty years the theory of the various carbonates seems to leave nothing more to be desired, I may venture to say that we have not even yet any certain rules for the caustification of the alkalies by lime.

I shall only announce at present, that numerous observations have proved to my brother and myself, that the potash furnished by the combustion of all the vegetables is a salt with excess of base, in which the proportions of carbonic

* I intend to publish, almost immediately, a memoir relative to the application of the alkali-metrical tube to proving the acetic, pyro-acetic, oxalic, and tartarous acids, &c. It will only be requisite to substitute in place of the mixture of sulphuric acid and water, an alkaline solution of a given alkali-metrical degree, and also to submit to trial a given weight of these acids with a vegetable base. The latter will probably one day act a very important part in the arts, and it will be important to be able to ascertain their energy easily, in order to be on our guard against the sophistications of some dealers.

acid

acid and potash are constantly the same; so that 100 parts of sulphuric acid always displace by their saturation only forty parts of carbonic acid, when we employ any of the potashes of commerce; while, if we saturate these same 100 parts of sulphuric acid by truly neutral carbonate of potash, we constantly displace 84 parts of carbonic acid. We know that heat takes from the carbonate of potash only a portion of its acid; I am therefore inclined to think that this quantity of carbonic acid remaining in the potash of commerce is constant, which even the heat of incineration cannot diminish; it is probable that, after having exposed carbonate of potash to this degree of heat, we shall find that it has not retained either more or less of carbonic acid than the potash of commerce, relatively to the alkali-metrical degrees respectively found in these salts.

Chemists who repeat these experiments will find that pearlashes give a less proportion of carbonic acid: it is probable that this is to be attributed to some circumstances of the process during which the ashes or the ley has absorbed a little less of the carbonic acid, which is both formed during combustion and is found in the atmosphere. This slight difference would explain why some manufacturers prefer the white potash of Russia to pearlashes. Thus, although a long exposure to the air may increase the proportion of carbonic acid in the potash of commerce, this does not prevent us from distinguishing two constant states of the carbonate of potash by applying this name to the truly neutral carbonate only, and by denominating the potash of commerce *supersaturated carbonate of potash:* this is conformable to the principles of the chemical nomenclature, as the borax of commerce with an excess of base has been called *supersaturated borate of soda.* I admit that between these two states of *carbonate of potash* and *supersaturated carbonate of potash* we may create intermediate degrees in all proportions, when by combining the carbonic acid with the latter we stop before having saturated all the alkali.

Some authors have designated as *alkaline carbonate of potash* the salt with an excess of base, to which I must call the attention of chemists; but although this denomination may

may seem at first view more natural, I think we ought to prefer that derived from the principles of the nomenclature, from which we cannot depart without replunging the language of chemistry into the confusion from which it has been so happily extricated: in fact, the carbonates of potash, soda, and ammonia, are alkaline carbonates, however neutral they may be: the denomination of alkalescent carbonate is not more proper, because this would indicate a successive diminution of the proportion of acid. The salt in question cannot therefore be called any thing else than *supersaturated carbonate of potash.*

Uncertainty of the Processes for the Caustification of the Alkalies.

I return to the caustification of the alkalis by lime: there can be no doubt as to the uncertainty which prevails on this subject, even in the most recent treatises in chemistry: I shall quote only one example among all those I might collect. We read in the *Systême des Connoissances Chimiques,* by M. Fourcroy, sect. iv. art. 10. no. 4, that "*in order to obtain pure potash we must mix it with double its weight of lime, and eight or ten times the weight of the total mixture of rain water*; we must boil them for two or three hours, filter and draw off the clear lixivium, this we must try by lime water, which it ought not to precipitate; *otherwise we should again pass it over quicklime.*" On the other hand, consult sect. v. art. 12. no. 16, and you will find what follows: "*we must mix the carbonate of potash with half its weight of quicklime,* upon which it must be strewed." Water should then be thrown over it in order to slake the lime; it is afterwards diluted; it absorbs carbonic acid; it passes to the state of carbonate of lime, which is insoluble, and the potash remains pure and caustic in the liquor."

[To be continued.]

XXXIV. *Re-*

XXXIV. *Report made to the Class of Physics and Mathematics of the Institute of France, upon a Proposition made by M.* Six, *Chief Director of the Fire-Engines of Paris, for substituting Water saturated with Sea Salt instead of common Water, for extinguishing Fires. By Messrs.* Chaptal *and* Monge *.

The advantages found by M. Six by employing the method he proposes are these:

1. To present a liquid which never freezes at the temperature of our climate.

2. To employ a liquid more proper than pure water for extinguishing fires.

3. To preserve for a longer time the casks, which are often broken from the water freezing in their insides.

4. To preserve the water from all decomposition.

5. To preserve the casks from the speedy destruction they suffer from fresh water left in them in a state of rest.

In order to give a precise idea of the merit of the method suggested by M. Six, we think it right to examine the two following questions:

1. At what degree of cold does water freeze when saturated with different quantities of sea salt?

2. Does water saturated with sea salt attack and destroy the leathern pipes employed in fire-engines?

In order to answer the first question, we submitted to a cold of —17° of Reaumur, produced by the mixture of pounded ice and bruised sea salt, some phials filled with a solution of sea salt at various degrees of saturation, from two up to twenty degrees.

This experiment was several times repeated, and always upon six kilogrammes of ice, with about half that quantity of pounded sea salt, and at a temperature of about ten degrees.

We constantly obtained the following results:

1. The solutions at two, three, and eight degrees were frozen some minutes after their immersion in the mixture.

* From *Biblioth. Phys. Econ.* for 1807, tom. i. p. 274.

The

The solutions above No. 12 were more tardy in freezing. When the solution was at 16 degrees, the freezing did not take place sooner than 40 or 50 minutes.

In the solutions at 19 or 20 degrees, only some few icicles are formed around the edges of the mixture.

2. At a cold of twelve degrees, no solution freezes in a mass; all of them from 2 to 15 degrees are frozen upon the sides of the phial, and the layer of ice becomes more or less thick, always in an inverse ratio to the degree of saturation. The solutions at 19 and 20 degrees never froze at all.

3. When the mixture of salt and ice is formed, and the thermometer is down at —17°, we may keep up this degree of cold for two hours at least, provided we slope the vessel so as to let the water flow out produced by the melting.

If we draw out the phials containing the solutions at the moment the thermometer begins to rise, we observe as follows:

1. The solutions which are below 10 degrees, present masses of ice only without any appearance of water, to such a degree of cold that these masses when carefully pounded do not, for some time, moisten the bodies upon which the operation takes place.

2. The solutions which mark from 10 to 16 degrees, present a soft kind of ice, which may be bent, and is of the consistence of sherbet.

3. The solutions which mark 18 and 20 degrees, present only some crystals of ice swimming in a limpid liquor, or fixed upon the sides of the vessels.

4. The thawing takes place more speedily in phials, when the liquor is at a high degree of saturation, and more slowly at the lower degrees, in such a manner that it follows vigorously and constantly the scale of saturation.

5. The icicles separated with care from the portion of water not frozen, and washed with pure water, present the following phænomena:

A. The water issuing from their melting is constantly salt.

B. The gravity of the water proceeding from the crystals of the solutions from 1 to 5 degrees, compared with that of the

the portion of the unfrozen solution, presented as follows:

In a flask containing 36 grammes of pure water, the water produced by the melting of the crystals No. 1 weighed 36 grammes 5 decigrammes; and the water not frozen, and carefully separated from the ice, weighed 36 grammes 6 decigrammes.

In the same flask, the water proceeding from the melting of the crystals No. 1 weighed 39 grammes 6 decigrammes; and the water not frozen weighed 39 grammes 8 decigrammes.

Although the results thus presented are not all necessary for answering the proposed question, we have thought it proper to report them all, in consequence of their appearing in opposition in some respects to the ideas hitherto entertained as to the freezing of salt water, and as they serve to clear up the processes followed in some northern districts for the extraction of salt from sea water, and perhaps tend to review and verify the theoretical principles of freezing.

The second question remaining to be examined, respects the injury that may be done to leather pipes by salt water.

In order to decide this question, they thought it best to be guided both by experiments and observation. They were informed by M. Bonjour, the director of the national salt works, that in the salt works of Meurthe they have for several centuries used, for raising the salt water, pumps in the cylinders of which round pieces of leather have been fixed; and that notwithstanding the uninterrupted usage of these pumps, and the contact of water saturated at 16 degrees, no alteration nor deterioration had taken place which could be attributed to the action of this liquid.

But even if it were true, that the saturated solution of salt attacked the leather or rendered it brittle, yet we do not think salt water should be rejected on that account, because M. Six with justice observes, that in the present state of the service they carefully wash the fire pumps and their pipes as soon as they have been used, in order to clear them from any

any impurities that may be deposited by the muddy waters of rivers, or the putrid water from casks.

Salt water, therefore, will not require any new precautions on the part of the firemen, and the leather of the pipes will not be moistened with it long enough to injure them. The reporters therefore are of opinion that the suggestions of M. Six present real advantages, and deserve to be adopted.

XXXV. *Report of Surgical Cases in the City Dispensary, Grocers-Hall-Court, Poultry, from the Beginning of March to the End of June* 1807: *with Remarks on the Propriety of establishing a Fund for the Relief of the Ruptured Poor. By* JOHN TAUNTON, *Esq.*

IN the last surgical report (see Philosophical Magazine, vol. xxvii. no. cvii. p. 236.) there were 144 patients under cure, from which period, during the four succeeding months, ending in June, there were admitted 473 persons.

Cured - -	392
Relieved - -	11
Died - -	2
Under cure - -	212
	617

Having noticed the frequent occurrence and danger of hernia (vol. xxvii. p. 236), which do not appear to have been sufficiently appreciated, or funds would have been raised in different districts for the purpose of affording relief to those who have it not in their power to relieve themselves; whether the want of ability arise from pure indigence, or from indiscretion, it must be admitted that the afflicted are entitled to the charitable consideration of a humane public.

In this metropolis (which is truly characterized as being pre-eminent to all cities in the world on account of its charitable institutions) it certainly appears that this disease has not passed unnoticed, as a "Society for the Relief of the Ruptured

Ruptured Poor" was established some years ago: another society of a similar nature has been advertised but a short time since. From these circumstances it might appear to a superficial observer, that the public have already done enough in providing for persons afflicted with this malady; but in proportion as these societies increase in number, not in magnitude, will their benefits be felt by diseased individuals, it being by the ready and easy access by which relief can be obtained, that the subject is rendered of more real public utility. Hence, if small funds were raised, and connected with every public dispensary, and if they were even extended to every parish throughout the kingdom (provided there was resident in that parish a medical man of science and humanity who would undertake to superintend free of all pecuniary rewards), "it would be a saving to the community at large, by the prevention of accidents, which always tend to increase the parochial rates."

This plan was recommended to the governors of the Eastern Dispensary by Mr. Luxmore, surgeon, at my suggestion, and has been adopted with much advantage to the poor in that district.

A subscription has also been set on foot, which may or may not be connected with the City Dispensary, but is equally calculated to afford relief to the afflicted supplicants, by giving to them the easiest possible access; and for every guinea subscribed the subscriber will be privileged to recommend three patients for *single* trusses, or one patient for a *double* truss.

JOHN TAUNTON,
Surgeon to the City and Finsbury Dispensaries, Lecturer on Anatomy, Surgery, &c.

Greville-street, Hatton-garden,
August 18, 1807.

Subscriptions and donations are received for the above establishment by James Amos, Esq. Devonshire Square, Bishopsgate; Mr. Elliott, at the City Dispensary; Mr. Bartlett, at the Finsbury Dispensary, St. John's-street; and Mr. Taunton, where plans may also be seen.

XXXVI. *Additional Memoir upon living and fossil Elephants. By M.* Cuvier*.

[Continued from vol. xxvi. p. 313.]

Article II.

Upon the Teeth of Elephants in general; their Structure, Growth, Succession, and Differences, with respect to Age and Position.

The manner in which these teeth grow and succeed each other is so extraordinary, they present in their various states, figures and sizes so variable, that it is not astonishing they have been so often mistaken.

We have made the following observations upon the two Indian elephants we had occasion to dissect; but we ought to acknowledge that we were guided by the excellent work of our colleague, M. Tenon, upon the teeth of the horse. Any thing peculiar we have seen in those of the elephant belongs only to their size and to their peculiar configuration.

We ought also to acknowledge that excellent observations have been already made upon the particular subject of the teeth of the elephant by M. Pallas †, Peter Camper and his son Adrian ‡, Messrs. Corse, Home §, and Blake ||: these three last, in particular, have almost exhausted the subject, each of them having of himself discovered some important facts.

As to the manner in which teeth in general arise and grow, our observations seem to us to confirm the theory of Hunter rather than that of any other author, in so far as regards that part of the teeth which has been called the osseous substance. But this great anatomist does not seem to us to have been equally happy as to the enamel; and he has entirely mistaken the nature of the third substance, pe-

* See Philosophical Magazine, vol. xxvi. p. 158. 201. 302.

† Acad. Petropol. Nov. Com. xiii. p. 472.

‡ Descrip. Anat. d'un Elephant.

§ Phil. Trans. for 1799.

|| Essay on the Structure and Formation of the Teeth in Man and various Animals: by Robert Blake, M.D. Dublin 1801, 8vo.

culiar

culiar to certain herbivorous animals. In these two last respects Mr. Blake seems to have approached nearest the truth; at the same time we are not of opinion, with him, that there are any vessels in the osseous substance.

In fact, each grinder of the elephant, like any other tooth whatever, is produced, and as it were conceived, in the interior of a membranous sac, which we shall call, in common with several anatomists, its *capsule*.

This sac, when viewed externally, is in the elephant of a rhomboidal form, not so high behind as before; it is close on all sides, if we except the small openings for the passage of the nerves and vessels.

It is lodged in an osseous cavity, of the same form with itself, in the maxillary bone, and which is to form one day the alveolus of the tooth.

It is only the external lamina of the capsule which has the simplicity of form we have mentioned. Its internal lamina forms, on the contrary, as in herbivorous animals in general, several folds; but in order to understand them we must describe another part.

I here mean the *pulpy nucleus* of the tooth; in each animal it has a peculiar figure: in order to conceive that of the elephant, let us imagine to ourselves, that from the bottom of the capsule, taken as a base, there issue kinds of small walls, all of them parallel, all of them transverse, and proceeding towards that part of the sac which is ready to leave the alveolus.

These small walls only adhere to the bottom of the capsule; their opposite extremity, or, if we please, their summit, is free from all adherence whatever.

This free summit is much thinner than the base; we may call it their edge; it is moreover deeply furrowed on its surface with several very sharp points or notches.

The substance of these small walls is soft, transparent, very vascular, and seems much to resemble gelatine in its nature; it becomes hard, white, and opake, in spirit of wine.

We may also easily figure to ourselves the folds of the internal membrane of the capsule; let us suppose that it forms

 productions

productions which penetrate into all the intervals of the small gelatinous walls I have described. These productions adhere to the face of the capsule which answers to the mouth and to the two lateral faces, but they do not adhere to its bottom, whence arise the small walls or gelatinous productions. Consequently, we may conceive a possible and continuous vacuum, although infinitely folded upon itself, among all these small gelatinous walls (descending for the upper teeth and ascending for the lower ones), and these small membranous partitions (ascending in the upper teeth and descending in the lower ones).

It is in this conceivable vacuum that the matters deposit themselves which are to form the teeth, viz. the substance vulgarly called *osseous*, which will be transuded by the gelatinous productions coming from the bottom of the capsule, and the enamel which will be deposited by the membranous partitions, and in general by the whole internal surface of the capsule and of its productions, the base alone excepted.

We must, however, remark, that between this supposed osseous substance and the enamel there is also a very fine membrane which I think I have discovered. When there is not as yet any part of the first substance transuded, this membrane envelops immediately the small gelatinous wall, and confines it more closely.

In proportion as this small wall transudes this substance, it retires inwards and removes itself from the membrane, which serves it nevertheless always as a tunic, but it is a tunic common to it and to the matter which has transuded under it.

The enamel of its side is deposited upon this tunic by the productions of the internal surface of the capsule, and it compresses it in such a manner against the internal or osseous substance, that the latter separates from it; and this tunic soon becomes imperceptible in the hardened portions of the tooth, or at least it does not appear, except upon the section, like a very slender grayish line, which separates the enamel from the internal substance. But we then always see that it is the latter alone which attaches these hardened

parts

parts to the bottom of the capsule; for without it the continuity would be broken.

The substance called *osseous* and the enamel are therefore produced by a kind of juxtaposition; the former is formed in layers from the outside to the inside; the interior layer is the last formed, and it is also the most extended, being absolutely the same as in shells; and its formation commencing by the most salient points of the gelatinous nucleus of the tooth, it is at these points that this substance is thickest; it goes on becoming thinner in proportion as it removes from them.

If we recur in our imagination to the epoch when this transudation takes place, we may conceive that there is formed a small cap upon each of the notches which divide the edges of the small gelatinous walls already mentioned. In proportion as new layers are added to the former, the caps change into conical horns; if the new and interior layers descend to the bottom of the scars of the edges of these small walls, all the little horns unite into one single transversal lamina; lastly, if they descend to the bottom of the small walls themselves, all the transversal laminæ will unite into one single corona of a tooth, which would present the same eminences and the same sections which we see in its gelatinous nucleus, if during the time these layers were transuding, other substances were not deposited above, and had not partly filled the intervals.

At first the enamel is deposited, as I have said, upon the surface of the substance called *osseous*, by the internal membrane of the capsule, under the form of small fibres, or rather of small crystals, all of them perpendicular to this surface, and forming on it in the first stages of dentition a kind of velvet, with fine hairs. When we open the capsule of the germ of a tooth, we find small molecules of the future enamel as yet very slightly adhering to the internal face of this capsule, and detaching themselves from it very easily. A part of these molecules also swims in a liquor interposed between the capsule and the germ. I never saw the small vesicles adhering to the capsule from which Herissaut asserts the matter proceeds, that afterwards, upon drying, becomes

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the enamel. The opinion of Hunter, that the enamel is only the sediment of the liquid interposed between the tooth and its capsule, is inaccurate, inasmuch as it keeps too far out of view the capsular membrane, whence in reality the molecules of the enamel issue; but it is very true, that these molecules are at first between this membrane and the tooth, before glueing themselves to the latter. As to the other opinion, which makes the enamel come out as if by efflorescence from the pores of the osseous substance; although it has been adopted by several anatomists, it has no foundation whatever. But to return to our teeth.

A thick layer of enamel thus coating the corona on all sides, fills a part of the intervals which the transversal laminæ and their notches had at first left between them.

The rest of these intervals is completely filled up by a third substance, which Mr. Tenon has called *osseous cortical*, because it envelops all the others, and resembles a common bone in its chemical nature and hardness, still more than the two other parts of the tooth. Mr. Home calls it *bone*, while he gives the name of *ivory* to the substance vulgarly called *osseous*. Mr. Blake gives to this cortical substance the name of *crusta petrosa*.

There is something very remarkable in its production. Mr. Tenon thought it proceeded from the ossification of the internal lamina of the capsule, after it has produced the enamel. Mr. Blake thinks that this lamina, after having given the enamel by one of its faces, gives the cortical by its opposite face. Mr. Home does not express himself clearly upon this subject.

For my part, I am convinced that the cortical is produced by the same lamina, and the same face which has produced the enamel: the proof of this is, that this lamina remains outside of the cortical, as it was formerly outside of the enamel, and that it remains there soft and free as long as this cortical gives it room. It changes in its texture alone; while it was producing enamel only it was thin and transparent. In order to produce cortical, it becomes thick, spongy, opaque and reddish. The cortical when growing is not like threads, it rather resembles small drops thrown out as if by chance.

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The membranous productions of the capsule of the tooth retire towards the top and the sides in proportion as the cortical which they deposit upon the enamel fills all the vacuum which remained between the different laminæ of the tooth. The peaks of these laminæ are covered with cortical like the rest, while they are not in use. One and the same production of the capsule often deposits its cortical upon the top of the lamina, which it again deposits upon the enamel, only at the bottom. It happens also that the top of the interval of the laminæ is already heaped up by the cortical when the base is still separated: in this case, the bottom of the capsular production is separated from the top, and does not receive its nourishment any longer, except from its lateral adherences with the capsule.

The deposition of the enamel begins almost with the transudation of the osseous substance, and that of the cortical follows closely after, in such a manner, that the summit of each lamina is terminated in its three substances long before its bottom, and that its adjoining laminæ are soldered together by their summits before being as yet hardened at their bases.

We may add to all this, that these various operations are not executed at the same time in all parts of the tooth, but they take place rather sooner before than behind. We may easily imagine that the anterior laminæ will be already united among themselves by their summits and even by their bases, when the intermediate laminæ will be still separated from each other, at least by their bases; and when the posterior laminæ are not even formed, and present only pointed and distinct horns, which should form the summits of their notches.

It therefore results from all we have advanced, that the substances of which the teeth are composed are all formed by excretion and by layers; that the internal substance in particular has nothing in common with ordinary bones, except in its chemical nature, being equally formed of gelatine and of calcareous phosphate; but that it neither resembles them in its texture, in its manner of forming itself, nor in its growing. Its texture presents neither cellulosity nor

 fibres,

fibres, but only laminæ cased within each other: those who compare it to the diploë of the cranium, and suppose there are cellulæ in it, give a very false idea of it. It is not formed in a primiture cartilaginous nucleus, which would be successively penetrated by earthy molecules; it does not grow by a general and simultaneous development of all its parts, and by preserving one same form; in short, it is neither penetrated by vessels nor by nerves. Those who thought that the vessels of the pulpy nucleus pass into the body of the tooth, have been deceived; and much more were those who establish a passage of vessels from the periosteum of the alveolus into the mass of the roots. The smallest possible fibre of the pulpy nucleus does not pass to the substance called osseous; and the latter is connected with the rest of the body only by its mechanical and forked formation. Thus no part of the tooth is regenerated when it has been once drawn; and if broken or cracked teeth sometimes reconsolidate, it is only because new layers forming within, glue themselves to the exterior ones, and glue the latter among each other.

We shall also see new proofs of this when we come to examine ivory, and thence we shall refute the objections drawn from the diseases of the teeth; but, in the mean while, we may safely affirm, that it is very improperly that several anatomists have given to the internal substance of the teeth the name of *osseous substance*, and equally improperly have they given the name of *ossification* to the operation which develops and hardens them: this is to confound two things essentially different, and to give, by ill applied names, false ideas which may even have an influence upon practice.

But let us now return to the grinders of the elephant.

[To be continued.]

XXXVII. No-

XXXVII. *Notices respecting New Books.*

The Chemical Pocket Book; or, Memoranda Chemica: arranged in a Compendium of Chemistry. By James Parkinson. *Fourth Edition; with the latest Discoveries.* 382 Pages, 8vo, 1807.—Symonds.

That one of the most useful compendiums of the most useful science should have passed through four editions in a few years, is a circumstance perhaps not less honourable to the taste and discrimination of the English public than to its author. It is, indeed, a pleasing proof of the growing taste for chemical knowledge, and of the general utility of this science to the conveniences and even necessaries of social life. Mr. Parkinson, in this fourth edition of his excellent Pocket Book, has not only added all the recent discoveries and observations made in chemical researches, especially in subjects connected with the arts and manufactures, but has greatly improved it in perspicuity and scientific arrangement. The neutral salts, particularly the nitrates, are very properly arranged according to the order of the affinities of their respective bases with nitric acid; a classification which, while it assists the memory, tends considerably to familiarise the mind of the young chemist with the important action of the affinities, which produce such extraordinary changes in almost all bodies. The additions and improvements, indeed, throughout every page of this interesting compendium are often highly important, as relating to the arts of dyeing, gilding, tanning, &c. &c., and sufficiently numerous, as will appear in the following extract from the article on sulphur, which is now much more complete than formerly. In addition to a description of all the sulphurets, the author has added that of the hydrosulphurets.

"Sulphuretted hydroguret of potash of Chenevix, hydrogenated sulphuret, &c. of Berthollet, and hydrosulphuret, &c. of others, is formed by impregnating a solution of the basis with sulphuretted hydrogen during the solution of sulphuret of potash and soda. The compound is crystallizable in transparent crystals; the solution, whilst recent, being colourless.

colourless. If sulphuric, muriatic, or any other acid which does not act upon hydrogen, be added to this solution whilst recent and colourless, the sulphuretted hydrogen exhales, but no precipitate is formed; but when, by standing, decomposition has taken place, and the solution is become of a greenish colour, hydrogen having escaped in union with the oxygen of the atmosphere, the sulphur thus abandoned by hydrogen is increased in its proportion, and is gradually converted into sulphurous acid: if then sulphuric or muriatic acid be added, sulphuretted hydrogen gas is exhaled, and sulphur is thrown down.

"Sulphuretted hydroguret of soda is obtained in a similar manner, and possesses similar properties.

"Sulphuretted hydrogurets of lime, barytes, strontia, magnesia, &c. may also be formed, possessing analogous properties.

"Sulphuretted hydroguret of ammonia s formed by passing sulphuretted hydrogen through ammonia. It does not possess the fetid odour of sulphuretted hydrogen, and is capable of crystallization.

"By pouring hydrosulphuret of potash gradually into muriatic acid, a portion of the sulphur combines with the sulphuretted hydrogen, and forms a substance resembling a yellow oil, which falls to the bottom, and is a supersulphuretted hydrogen, or the hydroguretted sulphur of Chenevix. Immediately on the solution of sulphurets taking place in water, a decomposition of the water commences, and sulphuretted hydrogen is formed; and this acquiring an additional dose of sulphur, forms the hydroguretted sulphur, which, by uniting with the base, forms an hydroguretted sulphuret; the sulphurets being thus changed, by solution, into hydroguretted sulphurets. On this principle are formed hydroguretted sulphurets of the different alkalies and earths.

"Hydroguretted sulphuret of potash may be obtained by boiling the alkali and sulphur together in water. The solution is of a greenish colour, of an acrid and bitter taste. It rapidly absorbs oxygen, and in close vessels deposits sulphur. It acts powerfully on the metals, it often reduces them to the state of sulphuret, being capable even of dissolving gold.

"Hydro-

"Hydroguretted sulphuret of soda is obtained in a similar manner.

"Hydroguretted sulphurets of barytes and of strontia are yielded by the solution of their sulphurets after exposure to the air. Their powers are much less than those of the other similar compounds.

"Hydroguretted sulphuret of lime is formed by boiling lime with sulphur in water. The solution is the only liquid capable of dissolving nitrogen gas in a notable quantity.

"Hydroguretted sulphuret of ammonia, or, as it was formerly called, Boyle's or Beguine's fuming spirit, or volatile liver of sulphur, is obtained in the form of a yellow fuming liquor, by the ammonia and sulphur uniting, whilst in a state of gas, during distillation, from one part of sulphur, two of ammonia, and six of quicklime. Like the other sulphurets it may be decomposed by acids; and if the concentrated sulphuric acid is employed, a dangerous degree of heat and explosive effervescence will be produced.

"Thus it appears that the simple union of sulphur with an alkaline or earthy base forms sulphurets; the union of sulphuretted hydrogen with such bases forms the hydrosulphurets, or sulphuretted hydrogurets; and by the union of the supersulphuretted hydrogen, or hydroguretted sulphur, with similar bases, the hydroguretted sulphurets are formed.

"Sulphur at 140° or 150° Fahr. begins to attract oxygen, and at 180° or 190° manifests a faint blue light; but the heat accompanying this combustion is so weak, that the sulphur may thus be burnt out of gunpowder without inflaming it. At 300° the combustion is accompanied by a reddish light. At about 290° it is converted into vapour. In oxygen gas it burns extremely rapid, and with a most vivid light. During its combustion, oxygen combining with its acidifiable base, forms an acid more or less perfect, according to the greater or less rapidity of the combustion."

The sulphates are not less accurately detailed, and even the anhydrous sulphate of lime, which is destitute of water, and yields 44·88 acid, and 55·12 base, is not omitted.

As gas lights have of late been the subject of much conversation, we subjoin the following extract from the account of

of carburetted hydrogen, or hydrocarbonate gas, the weight of which is to common air as 480 to 1000. 100 measures of this gas require for their saturation 60 measures of oxygen gas. On the vapour of water being brought into contact with charcoal, this species of gas is formed, together with carbonic acid.

"The gas obtained by the destructive distillation of pit coal has been successfully applied to the purpose of affording light by Mr. Murdoch in 1792, when he ascertained that it could be employed as a substitute for lamps and candles. It has been since used for similar purposes in France, forming what has been termed the *thermo lamp*. Mr. Henry, pursuing Mr. Murdoch's experiments, found that hydrogen gas, carburetted hydrogen gas, obtained by passing water over ignited charcoal, and the carbonic oxide, burnt with a very trifling production of light; and was induced to inquire into the cause of the difference between these gases and that derived from pit coal. The latter gas, when recently prepared, evidently contains inflammable matter suspended in it, which then increases its illuminating property; but this is subsequently deposited: still, however, the gas possesses the property of burning with a bright compact flame. The inquiries of Mr. Henry led him to the conclusion, that the light is in proportion to the quantity of combustible matter, and consequently to the quantity of oxygen consumed in the combustion. He therefore went through a series of experiments, by which he ascertained the quantity of oxygen gas required to saturate 100 measures of each gas, as well as the quantity of carbonic acid gas produced, as is shown in the following table:

Kind of Gas.	Measures of Oxygen Gas required to saturate 100 Measures.	Measures of Carbonic Acid produced.
Pure hydrogen - - -	50 to 54	
Gas from moist charcoal -	60	35
——— wood (oak) - -	54	33
——— dried peat - -	68	43
——— coal, or cannel -	170	100
——— lamp oil - -	190	124
——— wax - - - -	220	137
Pure olefiant gas - - -	284	179

"Now,

"Now, reckoning that for the production of each measure of carbonic acid gas, an equal measure of oxygen gas is employed, then by deducting the numbers in the third column from the corresponding ones in the second column, we find the number of the remaining measures which have disappeared in saturating the hydrogen of each gas; and as one measure of oxygen saturates two of hydrogen, double that number of measures is the volume which the hydrogen contained in that gas would occupy if expanded to its usual state. Thus, in the combustion of the gas from coal, of 60 measures of oxygen gas employed, 35 measures have gone to the formation of carbonic acid gas, and 25 measures have disappeared with the hydrogen; double this quantity, 50 measures, is therefore the quantity of hydrogen contained in this gas. Agreeable to Mr. Henry's opinion, that the degree of illumination depended on the quantity of inflammable matter contained in the gas, he found that the quantity of light evolved by each gas was, as nearly as could be judged, in proportion to the quantity of oxygen required for its combustion and detonation in a close vessel: so that, agreeable to the foregoing table, the gas from moist charcoal manifested least splendour, whilst the olefiant gas exceeds all the rest in brilliancy as well as in violence of detonation.

"Mr. Henry also concludes that the inflammable gases are mixtures of a very few simple ones. Gas from coals he supposes to be hydrocarburet with some carbonic oxide, and a small portion, perhaps, of olefiant gas: from charcoal, carbonic oxide, with hydrogen and a little hydrocarburet; from oil and from wax, pure hydrocarburets; except that the first contains one-eighth, and the latter one-fourth, of olefiant gas. The hydrocarburets from ether and alcohol he also found contained this gas; from the various admixtures of which, he thinks, proceeds that difference which induced Mr. Cruickshank to suppose so many different species of carburetted hydrogen."

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An Inquiry into the Changes induced on Atmospheric Air by the Germination of Seeds, the Vegetation of Plants, and the Respiration of Animals. By Daniel Ellis. pp. 256, 8vo. Murray, 1807.

Perhaps no speculation in experimental philosophy has ever been more generally received, than the notion that plants in the process of vegetation purify the air; and since the discoveries of Priestley, the labours of Ingenhousz, and the rhymes of Darwin, not only philosophers but ladies have been delighted with their presence, under the influence of this pleasing fancy. The opinion was first promulgated by Priestley in 1771, and almost without examination has been adopted by all subsequent philosophers, excepting some few indirect objections, down to the present author, whose able inquiry will most probably consign it to the oblivion of vulgar errors. "The investigation of this subject was suggested to him, he states, by accidentally observing the spontaneous recovery of an animal in whom all the appearances of life had been suspended by drowning. The result of his inquiry terminated in a conviction, that although many great and important steps had been made, yet much hypothetical conjecture was blended with established fact, and many suppositions were admitted into our theories which but ill accorded with the structure and œconomy of the animal system." No reference is made to the theories of vegetation and respiration, as they must depend on our knowledge of the changes produced by living bodies on the air; a subject, it appears, with which we are yet very little acquainted.

Mr. Ellis pursues his investigation by examining "the Changes induced on the Air by the Germination of Seeds—the Vegetation of Plants—the Respiration of Insects, Worms, Fishes and Amphibious Animals; and of Birds, of Quadrupeds, and of Man. On the Source of the Carbon in Vegetables and Animals, by which the Changes in the Air are effected; and the Phænomena which arise from the Changes induced on the Air by the living Functions of Vegetables and Animals." The following is a brief abstract of his principal observations on germination:

Dried

Dried seeds, although exposed to heat and air, may be kept to any indefinite time, without effecting a change in the atmosphere, or undergoing any material disorganization. If, however, moisture have access to them, they immediately begin to swell and germinate. Four or five days steeping in water, at 46°, or even 48 hours in temperatures from 60° to 66°, occasion putrefaction, carbonic acid and carburetted hydrogen gases are produced, and the faculty of germinating is destroyed. Hence it is concluded that water alone is essential to germination, but if applied too long it disposes to putrefaction. Besides water, a certain degree of heat is necessary to the germinating process. Light has been considered another agent; but, contrary to the opinion of the abbé Bertholin, the experiments of Ingenhousz and Sennebier controvert this supposition, by showing that light occasions evaporation, generates cold, and thus retards vegetation. However, although water and heat, appear to be the only agents essential to the beginning of germination, after a certain period air becomes equally necessary. According to the experiments of Achard and others, nitrogen gas, which forms nearly four-fifths of the atmosphere, completely obstructs germination and the formation of carbonic acid. Barley is converted into malt when exposed to oxygen gas, which gradually disappears, and carbonic acid takes its place; hence oxygen is likewise essential to the process of germination. A great variety of experiments, however, in different countries seem to establish as a fact, that of all known gaseous bodies the usual composition of the atmosphere is that which most facilitates a vigorous germination, a superabundance of oxygen as well as nitrogen being injurious. From these data a curious and somewhat difficult question arises, which the author discusses with great precision and accurate knowledge. The result of all the experiments on germination proves that the quantity of carbonic acid produced is very nearly in proportion to the quantity of oxygen which disappears: hence it is asked, By what process does a pea immersed in water absorb oxygen and disengage carbon? Water alone neither absorbs oxygen nor gives out carbon; and did the pea absorb oxygen, it must

acquire

acquire an additional weight in proportion; which is contrary to the fact proved by direct experiment. Peas immersed in water by Cruickshank gave out carbon when no germination took place, and when they approached the putrefactive process, in which case the quantity of air was increased. But where germination took place, and oxygen disappeared and carbon appeared, the quantity of air was not increased but rather diminished. Hence it is inferred, that the oxygen which disappears is not all absorbed by the seed in germinating, but only such a quantity as is merely equivalent to counterbalance the loss of carbon which unites with the oxygen to form carbonic acid, and in this manner effects a change in the volume only, and not in the weight either of the germinating seed or the incumbent gas. Mr. Ellis concludes, that "in germination the seed does not form carbonic acid *from its own substance*, but furnishes only one of the constituent parts of it, namely, the carbon; and that when it does form this acid, independent of oxygen gas, it is only under a state of decomposition, or in circumstances where no living action is going on." It is also observed, that the quantity of carbonic acid produced does actually exceed in weight the oxygen that disappears; and that as "carbonic acid is necessarily a product and consequence of germination, it seems absurd to consider it at the same time as an existing principle and a cause." Heat, moisture, and oxygen, he has proved essential to the process of germination: but how these substances act on each other so as to produce carbonic acid he has not ventured to determine, as the disengagement of a new substance in this case cannot be accounted for as it can be in that of combustion. It may be alleged, however, that the water gives mechanical expansibility to the seed, that the heat then effects its expansion, and that in the latter process the carbon is brought to a state fit to combine with the oxygen, and thus form carbonic acid.

In the second chapter Mr. Ellis examines the changes induced on air by vegetation, in which he combats the opinion respecting the alternate emission of oxygen and nitrogen gases by plants, and observes, that "both physiologists and

and chemists seem, in this instance, to have satisfied themselves with contemplating at a distance the beauty of the *final cause*, instead of approaching to a nearer examination of the facts on which the opinion has been maintained." He argues thus:

"Against the opinion of the absorption and emission of gases by the leaves of plants when growing naturally in air, we have already, both on physiological and on chemical grounds, been induced to enter our protest. That the same substance, carbonic acid, should during the day be absorbed by the leaf, and decomposed within it as salutary, and during the night should be formed within the same leaf, and emitted from it as noxious, seems to be not only inconsistent but absurd. Where would be the advantage in the carbon of the acid being retained for twelve hours as food, if for the next twelve it must again be given out as excrementitious? Or where is there an instance, in the whole circle of existence, of a living agent not only first forming its own food, but feeding on its own excretions? If this carbon were during the day retained as food, whence comes *that* composing the acid which plants, when confined in a given bulk of air, are constantly forming? If oxygen gas, as these chemists suppose, be during the day constantly emitted, why does that gas gradually disappear as the process of vegetation proceeds? And why at last is none to be met with, although there is present an abundance of carbonic acid, out of which it is supposed to be formed? It has been proved that during the day carbonic acid, by the act of vegetation, is constantly forming; but if, at the same time, it be as constantly absorbed by the leaves, how can its presence be manifested in such quantity and in such progression as experiment evinces that it is? All these observations apply to the circumstances of plants growing naturally in air; when they are placed in water, other phænomena arise, from which have been drawn arguments in favour of an absorption and emission of gases by leaves. It has, however, been shown by direct experiment, that when plants are confined in a given bulk of atmospheric air, they gradually

and completely destroy its oxygenous portion; which could not possibly happen if they possessed the power of emitting oxygen gas. The experiments, indeed, of Dr. Ingenhousz himself teach us, that this supposed emission of oxygen gas does not depend so much on the power of the leaves as on the quality of the water in which they are immersed; for, if the water be previously boiled, little or no oxygen gas is collected. Hence, then, we see, that to effect the separation of air from water the organized structure of the leaf is not only not necessary, but that the quality of the separated air is altogether different from what this supposed function of the leaves ought to supply. No proof, therefore, of the absorption and emission of gases, much less of oxygen gas, by the natural functions of leaves, can be derived from these experiments on plants immersed in water; and were the experiments even more precise, they would not in the least apply to the case of vegetables which flourish in the open air."

The third and fourth chapters embrace the subject of respiration. Scarcely an author from Ray to the present period, who has either directly or indirectly treated of this subject, whose opinions or observations are not here cited. The author's analysis of the numerous opinions and vague conjectures presents such an admirable specimen of deductive reasoning, as must ever be eminently useful to science and true philosophy. The addition or subtraction of a syllable in a verse would not be more sensibly felt than the omission of a section in this inquiry, which consists of 198. Mr. Ellis concludes that nitrogen gas is brought in contact with the respiratory organs of fishes and insects without undergoing any change; that oxygen is not absorbed by these organs, but, united to the carbon of the animal, forms carbonic acid; and that it is not to the presence of the (generally supposed deleterious) carbonic acid, but to the small proportion or total absence of oxygen gas, that the cessation of the animal functions is immediately to be ascribed."

An examination of the "Sources of Carbon in Vegetables and Animals, by which the Changes in the Air are effected," constitutes

constitutes the fifth chapter. In this difficult inquiry the author thus expresses himself:

"It is generally admitted that the cellular surface of the lungs is furnished with exhalant vessels. These vessels, like those of the skin and intestines, appear to be endued with a power, not only of exhaling water, but likewise of emitting carbon; for water and carbonic acid are expelled from the lungs in respiration in the same manner as they are produced by the skin when in contact with atmospheric air. As, therefore, the products of respiration and perspiration are in kind precisely similar, we are justified in ascribing their formation to similar laws; and since it seems to have been demonstrated, by direct experiment, that no transpiration of aëriform fluids takes place through the skin, we may presume that none is able to be carried on through the cells and blood-vessels of the lungs. Not only is the cellular substance of the lungs furnished with absorbent and exhalant vessels like that of the skin, but it is supplied from within by the same blood, and exposed from without on the same atmospheric air. It has been shown also, that the colourless fluids of various animals are able to effect the same change on the air as that which is produced by the blood; and that the serum of the blood itself (which is especially destined to supply the exhalant function) produces on the air the same identical change as it experiences in the lungs; all which circumstances strongly incline us to suppose that the function of the lungs resembles in kind that of the skin. The proofs likewise already adduced, that the carbon furnished by vegetables and by the inferior animals, as well those which perspire by the skin as those which breathe by lungs, depends wholly on the due circulation of their fluids, and is, consequently, the result of a living action, are strong presumptive evidence, that the same law obtains in the superior animals and in man; and seem to authorize the conclusion, that the carbon supplied in human respiration is truly an animal excretion carried on by the exhalant vessels of the lungs; and therefore that it primarily depends, like other excretions, on the due circulation and distribution of the blood, and is more or less affected by all its variations.

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In all animals carbon is a necessary constituent substance, and the means of acquiring it must be as constant as its expulsion during living action has been shown to be; and from no other source than through the organs of digestion and secretion can it be conceived to be derived. To these organs of digestion, assimilation, and secretion alone, are we enabled to trace it; but our knowledge of the theory of these functions in animals as in vegetables is extremely limited and imperfect. We have, indeed, of late succeeded in getting rid of much error and absurdity, but *have not, in any instance, attained to complete knowledge.*"

The last chapter of this excellent and logical inquiry investigates "the Phænomena which arise from the Changes induced on the Air by the living Functions of Vegetables and Animals." The general inconsistency and irreconcilcableness of the various opinions and experiments on this subject have not deterred Mr. Ellis from selecting the principal facts which tend to explain the phænomena of respiration and animal heat.

"Animal heat," says the author, "in all the amphibia mentioned, whether they inhabit the air or the water, seems to follow nearly that of the medium in which they are placed; and their standard temperature cannot, in consequence, be restricted to any fixed point, but must be considered always in relation to that of their surrounding medium. Nevertheless, the low degree of heat which these animals possess, is a proof that they have within themselves a power of producing heat. The loss of heat which insects suffer under cold, the fall of temperature in worms under melting snow, demonstrate that the surrounding medium, whether it be air or water, is constantly drawing off their heat; which renders necessary as constant a reproduction of it. It is also evident, from the experiments detailed, that during a state of torpor the temperature even of the warm-blooded animals exceeds only in a small degree that of the atmosphere by which their torpor is induced. But on passing into this torpid state, under which the temperature so greatly falls, the motion of the blood in animals gradually declines, and at length, in some cases, wholly ceases. All the

the secretory functions of the animal must, at this period, be suspended, in consequence of which the air in contact with it undergoes no change; but when heat is restored, the blood again renews its motion, the secretory functions return, and the air undergoes its accustomed changes. These changes consist in the conversion of its oxygen gas into carbonic acid, by carbon emitted by these animals through the medium of their respiratory organs. By these means a quantity of the specific caloric of that gas is at the instant set free; and to this constant liberation of caloric by the perpetual decomposition of the air, do we ascribe that superiority of temperature above the surrounding medium which those animals, as well as vegetables, during the continuance of living action are enabled to exhibit and preserve.

"In what manner, then, does the air breathed by the superior animals give out its heat to support that high degree of temperature above the surrounding medium, which they all possess? We have seen reason to conclude that the inspired air is decomposed in the bronchial cells of the lungs (which contain a superficies more than ten times greater than that of the whole body), and that all its oxygenous portion, which disappears, is converted into carbonic acid by carbon emitted from the exhalant surface of those organs. During this gradual conversion of the oxygen gas, a quantity of specific caloric, much greater than what is necessary to maintain the elasticity of the carbonic acid that is formed, is necessarily set free; and to this excess of heat, thus constantly liberated in the lungs by the decomposition of the air, do we look as the source of that superiority of temperature above the surrounding medium, which man, and other animals, under every vicissitude of climate, are enabled to exhibit and maintain."

The author proposes to pursue this inquiry, provided the present volume meets the approbation of the public. This we think certain, and cannot doubt that he has long since received sufficiently flattering encouragement to induce him not to suspend his important researches. He modestly states that he has "got rid of much error and absurdity, but has attained nothing complete;" but "the extensive series of

 facts

facts which he has brought together, and the analogies which, from the evidence of experiment, he has endeavoured to trace among them, may direct the attention of future inquirers to a more comprehensive view of the subject than has yet been taken, and impart a new degree of interest and utility to the research. The attempt also to combine the demonstrations of anatomy with the chemical phænomena which we observe, and to consider both in connection and subservience to the laws which characterize living beings, will, he trusts, meet with the approbation of physiologists, and tend to reduce within proper limits the application of chemistry to this science." In a word, we hazard nothing in saying that this is one of the most perfect analytical treatises that have appeared in modern days; that the author's style is nervous, concise, and conspicuous; and that with a complete knowledge of his subject, his inductive arrangement of experiments and facts is well calculated to establish a conviction of their accuracy and truth, and inspire respect for this fascinating but difficult branch of chemical science. We would recommend the author to add the dates to the experiments he has quoted, in his next edition, and thus present his readers with a brief but faithful historical view of the progress of the human mind in this interesting department of experimental philosophy.

XXXVIII. *Proceedings of Learned Societies.*

FRENCH NATIONAL INSTITUTE.

The following prize questions have been announced by the above learned body:

"A great number of substances give out, in different circumstances, a phosphorescent light, more or less lively, and more or less durable: such as the fluate of lime, and some varieties of phosphate of lime, when we throw them in powder upon a heated body; the Bolognian phosphorus, when, after having been exposed to the light, it is carried into a dark place; certain sulphurets of zinc, when rubbed

with

with a hard body, or even with the body of a quill; rotten wood, some kinds of fish, and other animal substances which approach putrefaction, when in a dark place, &c."

The Class of Mathematical and Physical Sciences proposes as the subject of a prize in physics, which it will adjudge at the public meeting on the first Monday of January 1809, the following question:

"To establish, by experiments, what are the relations which exist between the different modes of phosphorescence, and to what cause is each kind owing; excluding the examination of the phænomena of this kind observed in living animals."

The prize for the best memoir upon this question is to be a gold medal of the value of 3000 francs.

The memoirs must be sent to the secretary of the Institute before the 1st of October 1808.

The Class of Mathematical and Physical Sciences proposed in the public meeting before last the following question, which they once more lay before the public:

"To determine, by anatomical and chemical observations and experiments, what are the phænomena of the torpidity which certain animals, such as monkeys, hedge-hogs, &c. experience, during winter, with respect to the circulation of the blood, respiration, and irritability: to ascertain what are the causes of this sleep, and wherefore it is peculiar to these animals."

The memoirs on the above subject must be sent to the secretary's department of the Institute before the 1st of October 1807.

Conditions of the Competition for the above Prizes.

Any person whatever may become a candidate for these prizes; members of the Institute alone excepted.

None of the works transmitted may bear the author's name, but merely a sentence or device. A separate and sealed note may be attached, which may contain the name and address of the author, along with his device: this note will not be opened unless the memoir accompanying it merits the prize.

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The memoirs intended for competition must be sent to the secretary's department of the Institute free of expense, and the secretary will give receipts for them if required. They may likewise be sent, free of postage, to the perpetual secretaries of the mathematical and physical class of sciences.

The competitors are informed that the Institute will not return any of the works sent to it on the above occasion. The authors may therefore keep copies of them if they please.

The Institute will deliver the gold medals to the bearers of the receipts; and where there is no receipt produced, the medal will be transmitted to the author himself, or to some person appointed by him to receive it.

PROCEEDINGS OF THE INSTITUTE FOR THE LAST HALF-YEAR OF 1806.

Analysis of the Labours in the Physical Department, as drawn up by M. CUVIER *.

By the orders of the society the limits of the report we are now about to make are confined to the period of six months only; but our report is not the less rich in interesting results from this circumstance. The numerous vacancies which have occurred this year among the members of the Class of Mathematical and Physical Sciences, by exciting a lively emulation, have produced a remarkable collection of works in the various departments of the natural sciences. We shall continue faithfully to observe the practice, hitherto adopted, of analysing these works at the same time with those of our colleagues; the history of the sciences requires it: these labours, although foreign in appearance, are almost always connected with ours by the identity of the objects of inquiry, and we almost always appropriate some part of them to ourselves; by repeating and varying the observations which form their bases, we have often occasion to appreciate their merits.

Messrs. Bosc and Silvester, the principal candidates in the agricultural department, have greatly improved upon some

* From *Mag. Ency.* for February 1807.

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very excellent manuscripts on the science of agriculture, and some works already published on particular branches of it. The important places with which government has intrusted them in this part of the administration, and their extensive acquirements in the physical sciences, have equally been taken into consideration: the class has had the pleasure of adopting both of them as its members. M. Silvester has succeeded M. Cels; and M. Bosc, a profound naturalist as well as an experienced agriculturist, and from whom we have derived many important works upon the history of animals, has taken the place of the veterinarist Gilbert, which has been vacant these five years.

In the botanical department there was no place to dispose of, except that of the late M. Adanson; but the competition has not been the less brilliant on that account, both in the number and importance of the works which the candidates have submitted to the judgment of the class. It ought to be a great satisfaction to the friends of the sciences to be informed of these striking proofs of the zeal of those who cultivate them.

M. Palisot de Beauvois, who was the successful candidate, had strong claims, founded on his travels in Africa and America, on his *Flora of Oware and of Benin*, which we have often mentioned as having added some singular plants to botany; on the Flora of the United States of America, which he is preparing, and of which he has already given some interesting specimens; and, lastly, on his long researches upon the subject of those plants commonly denominated *cryptogamia*. These researches partly consist of descriptions of new species, and in establishing genera or other methodical distributions, of which it would be difficult to give an extract; but they also comprehend more general objects, and chiefly a system upon the fecundation of mosses and mushrooms, which we think ourselves so much the more bound to analyse, because, although this fecundation has been for a long time announced in works in very extensive and well-merited circulation, botanists do not seem to have paid sufficient attention to it.

We know that the mosses produce at a certain period pe-

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dicles more or less long, terminated by capsules or urns of a very complicated organization, and filled with a dust of various colours.

Dillenius and Linnæus thought these capsules were antheræ or organs of the male sex, and they sought for those of the female sex in certain groups of leaves in the form of rosettes or stars, which we remark upon other parts of some of these small plants.

Their opinion however was never very prevalent; it was thought, on the contrary, that the dust which filled the urns was the seed, and not the pollen.

It then became necessary to inquire into the analogy of the stamina. Hill thought he saw it in the ciliæ on the edge of the urn; Kœhlreuter, in the hood; Schreber, in certain small threads at the bottom of the pedicle; and others were of the same opinion also.

But in 1774 a physician established at Chemnitz, John Hedwig, a name subsequently very celebrated, observing in the rosettes of some mosses some cylindrical bodies, which had been discovered a long time before by Micheli, perceived that they were open at the end, and that they emitted a powder of excessive tenuity: he did not doubt therefore that these were antheræ. Having afterwards sown the grosser dust which fills the urns, he saw moss spring up, and concluded that this dust was the grain, as several others had supposed before his time, and consequently that the urn was the fruit, or the female organ fecundated.

These observations were first published in an abridged form in 1777;—they were crowned by the academy of St. Petersburgh in 1781, and followed up for more than thirty years with most astonishing patience, and elucidated by copious works and by a great many designs made by the microscope; and they have obtained the approbation of almost all the botanists of Europe, particularly of those who make the mosses a particular object of study: the only objection, advanced very strongly at first, viz. that no rosettes were found in certain genera of mosses, has been nearly destroyed, since Hedwig, by dint of study, has succeeded in showing that the antheræ in these cases are in the buds of the axillæ of

of the leaves, or rather that they accompany the base of the pedicle of the urn: in short, he has shown them nearly in all the genera.

It is nevertheless this system, so strongly accredited, that M. de Beauvois combats, in order to substitute in its place a system which he presented to the Academy of Sciences of Paris in 1782, and of which the following is the foundation:

In the midst of this dust in the urns, which Hedwig regards as the grain, there is a kind of nucleus or small axis, more or less swelled, called by botanists a *columella*. Those who have observed it have seen nothing else in it than a parenchyme more or less cellular; Hedwig represents it several times in this manner: but M. de Beauvois says he has here remarked some very small grains, and thinks that these are the true seeds: the other dust which fills the urn around this nucleus, is according to him the pollen; the movements of the ciliæ of the edge of the urn, when these ciliæ exist, have only, as he thinks, for their object to compress the pollen against the seeds, in order to fecundate them at the moment they are about to escape. Thus, according to M. de Beauvois, the urn is hermaphrodite, and all the complicated apparatus of the organs taken by Hedwig for antheræ, and which is found in almost all the mosses, has no use that we know of: the individuals of certain species which bear rosettes only, take no share at all in propagation; the pollen is larger and more abundant than the seed: even the latter would have been invisible to almost every observer; it would be fecundated, not in the ovary, and while yet tender and small, as all other plants are fecundated, but at the moment of its escape, and when it is already completely developed: in short, if we ask how M. Hedwig produced mosses, by sowing what M. de Beauvois thinks is nothing else than the *pollen*, the latter answers, that Hedwig sowed this almost invisible but real grain at the same time with the pollen, without knowing it. It may be thought, that in order to confirm so novel an opinion, this grain should not only have been exhibited, but it should have been sown separately and detached from the other: unfortunately, however, this last experiment has not been made; and,

and, as we may judge from the above detail, it is even almost impossible to execute it.

[To be continued.]

XXXIX. *Intelligence and Miscellaneous Articles.*

VACCINATION.

Doctor Thornton's "Proofs of the Efficacy of the Cow-Pox," first published in this Magazine, has been translated by Dr. Duffour, an eminent physician in Paris, and has excited a great sensation over the whole of France. Affixed to this translation are testimonies in its favour from the most eminent persons of France: Prince Cambaceres; Corvisart, first physician to the emperor Napoleon; Cuvier, secretary to the National Institute; Thouret, president of the School of Medicine; the archbishop of Malines, the bishop of Nantz, and several of the præfects of departments: and the translation has been ordered by government to be distributed throughout the departments. In order to stamp still greater value on it, the National Institute has voted to have Dr. Thornton's work in their library, as have also the Faculty of Physic at Paris.

AMERICAN INDIAN ANTIQUITIES.

Mr. Jefferson, the President of the United States of America, is in possession of several busts carved by the Indians; in these the human form reaches down to the middle of the body, and they are nearly of the natural size. The traits are well marked, and characterize those which are peculiar to copper-coloured men; one of them in particular represents an old savage:—the wrinkles in his face, and his whole countenance is peculiarly expressive. These busts were found when digging in a place called Palmyra upon the river Tenessees. They have not as yet discovered of what they are composed; some people think they are formed of a solid stone cut with a chisel, while others think it is a moulded composition, and fired like porcelain. It is uncommonly hard.

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It is uncertain whether these busts represent idols worshipped by the natives, or some distinguished personages among them. It may be a subject of inquiry, who were the predecessors of the present race of Indians, who could thus execute such an excellent resemblance of the human form.

LECTURES.

The Autumnal Course of Lectures at St. Thomas's and Guy's Hospitals will commence as follows:

St. Thomas's.

Anatomy and the Operations of Surgery, by Mr. Cline and Mr. Astley Cooper, Thursday, October 1, at two o'clock.

Principles and Practice of Surgery, by Mr. Astley Cooper, Monday, October 5, at eight in the evening.

Guy's.

Practice of Medicine, by Dr. Babington and Dr. Curry, Friday, October 2, at ten o'clock.

Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen, Saturday, October 3, at ten o'clock.

Midwifery, and Diseases peculiar to Women and Children, by Dr. Haighton, Monday, October 5, at eight in the morning.

Pathology, Therapeutics, and Materia Medica, by Dr. Curry and Dr. Cholmeley, Thursday, October 6, at eight in the evening.

Physiology, or Laws of the Animal Œconomy, by Dr. Haighton, Wednesday, October 7, at seven in the evening

Experimental Philosophy, by Mr. Allen, to begin in November.

Clinical Lectures on select Medical Cases, by Dr. Babington, Dr. Curry, and Dr. Marcet.

N. B. These several Lectures are so arranged as not to interfere with each other in the hours of attendance; and the whole is calculated to form a complete Course of Medical and Surgical Instruction. Terms and other particulars to be learnt from Mr. Stocker, apothecary to Guy's Hospital, who is also empowered to enter gentlemen as pupils to such lectures as are delivered at Guy's.

Theatre

Theatre of Anatomy.

Mr. Taunton's Autumnal Course of Lectures on Anatomy, Physiology, Pathology, and Surgery, will commence at the Theatre of Anatomy, Greville-street, Hatton-garden, on Saturday, the 3d of October, at eight in the evening. In these Lectures it is proposed first to take a comprehensive view of the Structure and Œconomy of the living Body, and then to consider the Causes, Symptoms, Nature, and Treatment of Surgical Diseases, with the Mode of performing the different Surgical Operations. The Pupils will also have the opportunity of attending the Clinical Practice of both the City and Finsbury Dispensaries.

Particulars may be had on applying to Mr. Taunton, Greville-street, Hatton-garden.

Theatre of Anatomy, Blenheim-street, Great Marlborough-street.

The Autumnal Course of Lectures on Anatomy, Physiology, and Surgery, will commence on Thursday, the 1st of October, at two o'clock in the afternoon, by Mr. Brookes.

In these Lectures the Structure of the Human Body will be demonstrated on recent Subjects, and further illustrated by Preparations, and the Functions of the different Organs will be explained.

The Surgical Operations are performed, and every Part of Surgery so elucidated, as may best tend to complete the operating Surgeon.

The Art of Injecting, and of making Anatomical Preparations, will be taught practically.

Gentlemen zealous in the Pursuit of Zoology will meet with an uncommon Opportunity of prosecuting their Researches in Comparative Anatomy.

Surgeons in the Army and Navy may be assisted in renewing their Anatomical Knowledge, and every possible Attention will be paid to their Accommodation as well as Instruction.

Anatomical Converzationes will be held Weekly, when the different Subjects treated of will be discussed familiarly, and the Students' Views forwarded.—To these none but Pupils can be admitted.

Spacious

Spacious Apartments, thoroughly ventilated, and replete with every Convenience, will be open at Eight o'Clock in the Morning till Four in the Afternoon, for the Purposes of Dissecting and Injecting, where Mr. Brookes attends to direct the Students, and demonstrate the various Parts as they appear on Dissection.

An extensive Museum, containing Preparations illustrative of every Part of the Human Body, and its Diseases, appertains to this Theatre, to which Students will have occasional Admittance.—Gentlemen inclined to support this School by contributing preternatural or morbid Parts, Subjects in Natural History, &c. (individually of little Value to the Possessors) may have the pleasure of seeing them preserved, arranged, and registered, with the Names of the Donors.

The Inconveniencies usually attending Anatomical Investigations are counteracted by Antiseptic Process, the result of Experiments made by Mr. Brookes on Human Subjects, at Paris, in the Year 1782; the Account of which was delivered to the Royal Society, and read on the 17th of June 1784. This method has since been so far improved, that the florid colour of the Muscles is preserved, and even heightened. Pupils may be accommodated in the House.—Gentlemen established in Practice, desirous of renewing their Anatomical Knowledge, may be accommodated with an Apartment to Dissect in privately.

The Summer Course is continued every Morning at Seven o'clock.

Medical and Chemical Lectures, St. George's Hospital, and George-street, Hanover-square.

The first week of October a Course of Lectures on Physic and Chemistry will commence in George-street at the usual morning hours, viz. the Medical Lecture at Eight, and the Chemical at a quarter after Nine o'clock, by George Pearson, M. D. F. R. S., Senior Physician of St. George's Hospital, of the College of Physicians, &c.

Note, A Register is kept of the Cases in St. George's Hospital, and an account given of them every Saturday morning, at a Clinical Lecture, at nine o'clock.

METEORO-

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For August 1807.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
July 27	66°	73°	60°	29·98	66	Fair
28	67	79	67	30·01	88	Fair
29	67	74	66	29·70	76	Cloudy
30	65	74	64	·69	47	Cloudy
31	63	69	55	·61	15	Rain
August 1	60	72	57	·81	60	Fair
2	59	74	56	·89	55	Fair
3	65	73	57	·85	40	Showery
4	63	72	59	·87	55	Fair
5	60	69	58	·90	40	Cloudy
6	61	71	59	·85	32	Fair
7	62	69	60	·90	19	Rain
8	60	68	61	·90	18	Rain
9	61	63	56	·91	26	Fair
10	57	65	55	30·00	22	Fair
11	60	74	61	29·82	46	Fair, rain at night
12	62	72	64	·76	37	Fair
13	63	79	66	·72	51	Fair, great fall of rain at night
14	60	73	61	·68	34	Fair
15	62	71	66	30·00	35	Showery
16	67	76	66	·23	31	Fair
17	64	76	64	·17	42	Fair
18	64	75	60	·03	55	Fair
19	61	78	67	29·93	51	Fair
20	66	73	66	·92	30	Cloudy
21	67	78	67	·85	45	Cloudy
22	64	79	67	·84	55	Fair
23	69	78	67	·85	57	Fair
24	66	69	64	·87	0	Rain
25	61	74	60	29·93	38	Fair
26	62	74	61	30·00	40	Fair

N. B. The Barometer's height is taken at one o'clock.

XL. *Facts for a History of the Gallic Acid.* By *M.* Bouillon-Lagrange*.

Of all the vegetable acids, the gallic acid may be regarded as that which presents most interest; it has therefore been the object of the inquiries of several chemists. Macquer, Monnet, Lewis, Cartheuser, and Gioanetti, have pointed out the method in which substances called *astringents* operate upon solutions of iron. The academicians of Dijon were the first to ascertain the presence of an acid in these substances; and in 1772 they showed that the products distilled from gall-nuts blackened the solution of sulphate of iron, and that its infusion reddened turnsole tincture. These details were as yet nothing more than a general proof of the acid nature of the principle of gall-nuts, but they did not furnish the means of extracting and obtaining this acid separately; it is to Scheele we owe this discovery. His process was published in 1780. M. Deyeux, some years afterwards, (in 1793,) discovered that this acid might be obtained by sublimation. Messrs. Berthollet and Proust afterwards added by their researches to the knowledge we possessed already of the properties of this acid; so that we may now regard it as one of the best known vegetable acids.

Several foreign chemists have also given, within these few years, processes for the extraction and purification of this acid: but none of them, M. Richter excepted, have rivalled M. Scheele's. Among the number of experiments which chemists have made upon this subject, there is one which I have never seen either quoted or refuted in the various memoirs published upon the gallic acid.

We found in a letter of M. G. Charles Bartholdi to M. Berthollet, in the year 1792, some facts which deserve attention.

M. Bartholdi first points out a process for obtaining pure gallic acid; he afterwards treats this acid with metallic oxides. The author informs us that all the bodies which give up oxygen to the gallic acid make its colour brown; that,

* From *Annales de Chimie*, tom. lx. p. 156.

in these operations, the acid itself, by carbonizing, forms, after a slight combustion, colouring particles.

For this purpose he boiled red oxide of mercury for half an hour in a solution of gallic acid, which assumed a blackish colour. He found in the residue revived mercury mixed with a charry powder; he afterwards saturated the liquor with carbonates of potash and soda. These salts did not give him any blue precipitate with the sulphate of iron. He obtained a similar result with the oxide of manganese.

Other experiments persuaded the author that those bodies which take up oxygen from the gallic acid brighten its colour. "I have made (says M. Bartholdi) a solution of gallic acid as limpid as distilled water, having boiled it for some time with very pure and well pulverized charcoal, which I used in double its weight to that of the acid; it preserved its limpidity as long as I kept it from the influence of the atmosphere, and it precipitated the iron in a black colour."

M. Bartholdi presumes that we may thus succeed in destroying the astringent property.

I shall not indulge myself upon the present occasion in any observations; it will be necessary, in order to limit them precisely, to detail the following experiments:

Extraction of the Gallic Acid.

There are several processes for extracting this acid from gall-nuts.

Scheele's Process.—We pour upon one part of gall-nuts, pounded, and passed through a coarse sieve, six parts of cold water. We must then infuse it in a glass bowl for four days, taking care to stir it often; it must be then filtered, the liquor exposed to the free air in the same vessel, covered with a piece of gray paper only: a month afterwards we find this infusion covered with a thick mouldy pellicle, without any precipitate being formed; it has no longer any astringent taste, but it is acid. The liquor is now allowed to rest for five weeks; there is then formed a precipitate two fingers in thickness, and there is a mucous pellicle above it. The infusion is filtered, and again exposed to the air. At the end

end of some months the greatest part of the liquor has evaporated; all the precipitates are collected, and cold water poured above them; they are allowed to subside, and are then decanted: as much hot water is added as is necessary for solution; it is filtered, and evaporated in a gentle heat, and yellow crystals are obtained.

M. Bartholdi's Process.—We evaporate the alcoholic tincture of gall-nuts; we afterwards dissolve in distilled water, and add to the solution sulphuric acid until the mixture has acquired a decidedly acid taste; the extractive matter is precipitated in a few hours; and the supernatant fluid, freed from the sulphuric acid by means of barytes, gives, according to the author, pure gallic acid.

This process does not present this result. It is generally very difficult to catch the moment when all the acid is taken up by the barytes, considering that it is also combined with gallic acid. Nothing remains after the evaporation of the liquor, except a bitter substance, containing a great deal of tannin, and which is not susceptible of crystallizing.

M. Deyeux's Process.—This chemist discovered that, by heating slowly and cautiously in a glass retort pounded gall-nuts, there was sublimed a considerable quantity of lamellated brilliant crystals.

M. Richter's Process.—Digest gall-nuts, reduced into a fine powder, in cold water, taking care to stir the mixture frequently. After some time, the whole is pressed out through a linen cloth; the residue is mixed once more with water and subjected to the press: the liquors are mixed, and evaporated at a very gentle heat; a blackish brown and very brittle substance is obtained: this substance, when reduced to a fine powder and digested with very pure alcohol, yields a slight straw colour. The second infusion is almost colourless; it leaves a brown residue, which is almost entirely pure tannin. These two alcoholic liquors are mixed and distilled in a small retort to one-eighth. Upon cooling, the liquor congeals into a mass; water is then poured above, and the mixture is slightly heated: we then obtain a clear and almost colourless solution.

If we submit this solution to evaporation, we may obtain

from it very small and very white prismatic crystals; the mother water furnishes them also, but these are generally a little coloured; it is sufficient to wash them with water, in order to obtain them very white. We obtain by this process half an ounce of crystals for each pound of gall-nuts: these crystals are extremely light.

The processes of Scheele, Deyeux, and Richter, have given us the most advantageous results; but they differ with respect to the purity of their acid. The first, as M. Berthollet has observed, retains a great deal of tannin; the second is perfectly white, and the third contains tannin also.

By M. Richter's process, the acid, after having been purified, is of a straw colour. I tried, without effect, to bring it to that state of purity pointed out by the author: I ascertained that, if we wished to pursue the evaporation, desiccation, and afterwards the action of alcohol, we might each time decompose a certain quantity of acid, so that the alcoholic liquor, in place of being more transparent, would become brown. There is therefore a term at which we must stop, if we wish to preserve the totality of the acid as well as its properties.

M. Berthollet has attempted several methods of purifying Scheele's acid; the plan which succeeded best, was to treat this acid with oxide of tin recently precipitated from its solution by an acid.

I repeated this experiment. The following is the manner in which I operated, and the phænomena I observed:

After having separated the oxide from the muriate of tin by an alkaline base, it was washed clean with boiling water; it was then boiled for some time in a new quantity of water. It was afterwards treated with gallic acid, and evaporated to the consistence of thick honey. Distilled water was then added; the liquor, after having been filtered, was clear and limpid, and without taste or smell: when evaporated to dryness no produce was obtained.

This difference in the results obtained by M. Berthollet gave me reason to suppose that I had committed an error: I began the experiment again, and paid every possible attention to it.

I dissolved

I dissolved 61 grammes of gallic acid, confusedly crystallized and still very brown, in five hectogrammes of boiling water. I kept a part of this solution in order to compare it; the rest was submitted to ebullition with 61 grains of oxide of tin well washed and still humid; I evaporated until there remained about half of the liquor only, and I afterwards added a sufficient quantity of water to restore it to its primitive weight: I then compared them; the latter had lost its colour considerably. The difference in the degree of acidity was not sensible; it still precipitated glue. The precipitate of it was yellow and flocculent, while that by the unpurified liquor was brown, muddy, and more abundant; it even congealed into a mass. We see that the acid is not yet decomposed; but I could not obtain, like M. Berthollet, crystals so white and pure as those given by the process of sublimation.

Being desirous of knowing if a new quantity of oxide of tin would entirely deprive this acid of tannin, I added to the liquor 30 grammes of oxide of tin, and evaporated until there only remained about 100 grammes of liquor, which was poured off clear without colour; it did not precipitate either sulphate of iron or glue. I could not obtain gallic acid by evaporation.

This experiment proves that it is very difficult to deprive the gallic acid completely of tannin; and that, by pursuing the action of the oxide of tin, we decompose the acid. It is certainly in this manner that M. Proust has proceeded; because this chemist has observed in his Memoir upon Tannin, printed in the *Annales de Chimie*, tom. xlii. that the oxide of tin he used for purifying the gallic acid, only gave him as a product a liquor without taste or colour, making not the slightest impression upon the solutions of iron or tincture of turnsole.

As to the methods proposed by M. Bartholdi, I do not think they can be employed. However, as the author has neglected to examine the products of his operations, I thought it was necessary to repeat his experiments, and to determine the matter of the results to which they might give birth. For this purpose I poured a solution of gallic acid

upon the red oxide of mercury; it instantly became brown, and afterwards changed to black: the liquor then assumed a deep brown colour: in this state it was still acid, gave a blue colour to a solution of sulphate of iron, and precipitated glue; but it did not contain mercury.

I boiled this liquor over a new quantity of oxide; it then became clear, without colour, and did not contain either tannin or gallic acid.

A part of the oxide of mercury was reduced; the other was mixed with concrete phosphoric acid, but nothing was sublimed by the action of caloric.

If we use charcoal previously purified in place of red oxide of mercury, the solution of gallic acid loses almost entirely its colour and taste; the liquor becomes green, and does not precipitate glue; but it still gives a violet blueish tint to the solution of sulphate of iron. When boiled with a new quantity of charcoal the liquid becomes colourless, and produces no change in the solutions of glue and sulphate of iron. After having evaporated it to dryness, there remained in the capsule a brown substance, which precipitated the acetate of lead in a dirty gray colour, and the nitrate of mercury and muriate of tin in yellow; we may therefore consider it as being an extractive matter.

These experiments demonstrate that there exists no other process for purifying the gallic acid of Scheele, except sublimation; at least, that the proportions of oxide of tin employed by M. Berthollet, but which he has not pointed out, have not a great influence upon the result. The method of purifying it by sublimation, however, cannot be admitted if we wish to preserve all its properties. The different characters presented by these acids furnish the proof of this assertion.

Comparison of the Gallic, Crystallized, and Sublimed Acids.

Crystallized Acid of Scheele.

This acid gives to water a slight lemon colour; the solution becomes darker on the contact of the air; it reddens turnsole tincture: lime water produces a blue colour in it; an

an excess makes it pass to the colour of pea blossom; and if we add some drops of nitric acid, the liquor becomes rose-coloured. The same phænomena take place with barytes.

This solution assumes a colour more or less greenish with the carbonate of soda; with that of ammonia the colour does not change. It is a deep brown with caustic potash, and with ammonia it is of a reddish brown.

With the green sulphate of iron the colour is violet blue, which is constant with this sulphate; an excess does not change it. With the nitrate of mercury we obtain a yellow precipitate; it is white with the acetate of lead and the muriate of tin.

The solution of this acid presents no phænomenon with the oxymuriatic acid.

It forms an abundant precipitate with glue.

The same experiments have been made upon the acid obtained by M. Richter's process: the results were similar, with this sole difference, that it formed with glue a very small quantity of precipitate.

Sublimed Acid of M. Deyeux.

The solution of this acid in warm water liberates an aromatic smell, and we perceive at the surface of the liquor a slight oily pellicle.

Exposed to the contact of the air, this solution becomes brown. It reddens a little the tincture of turnsole; lime water gives it the colour of wine lees, and an excess changes it to the fawn colour; with barytes we obtain this last shade, and the liquor is immediately covered with an oily pellicle.

Carbonate of ammonia produces no change in the acid liquor; carbonate of soda gives it a fawn colour.

Caustic potash makes it brown; the colour is clearer with ammonia.

If we pour into this acid liquor some drops of a solution of sulphate of iron, we obtain a blue colour which soon becomes violet blue; very often also, in place of a blue colour, it is of a deep green. This proceeds, without doubt, from some particular circumstances. I think we may attribute this

 phænomenon

phænomenon to the state of oxygenation of the iron; because, with the muriate of iron at the maximum, we have constantly a green colour. This effect is less remarkable with the two other acids; the cold infusion of gall-nuts always retains its beautiful blue colour.

With nitrate of mercury the precipitate is blackish; that by the acetate of lead is fawn-coloured, and very light.

The sulphates of zinc, copper, and muriate of tin, present no phænomenon.

Oxymuriatic acid browns the solution of gallic acid, and an excess discolours the liquor.

On comparing the differences presented by these acids it will be easy to appreciate them.

The sublimed acid is less acid; it is decomposed in the air; it has no action upon barytes, upon carbonate of ammonia, nor upon muriate of tin. The precipitate obtained with the nitrate of mercury is blackish in place of being yellow; that of the acetate of lead is slight and fawn-coloured in place of being white and abundant.

Oxymuriatic acid browns a clear and transparent solution of the sublimed acid, while it does not change the colour of that of the crystallized acid.

In short, this acid does not give a constant colour with sulphate of iron, and does not precipitate glue.

If it be easy to demonstrate the characters which distinguish these two acids, it is otherwise when we wish to explain whence their difference arises. M. Berthollet has well ascertained that the acid of Scheele, not purified, contained a great deal of tannin; and he found that when purified by oxide of tin it did not precipitate glue.

As to M. Richter's process, I have pointed out above the analogy it has with Scheele's acid; nevertheless these two acids seem to me to differ from that obtained by sublimation; this last contains a small quantity of volatile oil, which is combined with it, and which with the help of caloric assumes a character which relates it to the resinified oils. We may ascertain this property by dissolving this sublimed acid either in ether or in alcohol; if we volatilize the liquid by rubbing

rubbing it on the skin, we feel an effect similar to that produced by a resin dissolved in alcohol.

It is not without difficulty, as may well be supposed, that we succeed in ascertaining correctly the nature of the gallic acid. Does this acid exist completely formed in gall-nuts? May we regard it as a particular acid; or rather, is it not the result of the combination of a vegetable acid with tannin, with extractive matter, or with other substances existing in gall-nuts? These are questions which still remain to be resolved. I endeavoured, by a course of experiments, to add some facts to those already known; and if they do not as yet lead to a complete solution, I think that we shall find in them some new results, at least, which will serve to ascertain the nature and properties of the gallic acid.

[To be continued.]

XLI. *Extract of a Memoir upon two new Classes of Galvanic Conductors. By M.* Erman, *of Berlin**.

The anomalies of the conductory faculty are so strongly evinced in galvanic electricity, that they have furnished arguments to those who have attempted to refer phænomena of this kind to a principle essentially different from electricity. The examination to which I submitted a great number of substances relatively to the phænomena they present, when we employ them to close the galvanic circle from one pole to the other of the pile, has furnished me with answers to some of these arguments: but I obtained from them a much more precious result; since I convinced myself that, in this kind of effects, all possible combinations are realised. A given substance being applied to the two poles of the pile, there constantly happens one of five things:

1. This substance, not acting separately upon any of the poles, leaves them therefore perfectly insulated, when we try to put them in conflict by its intermedium; whence it results

* From *Annales de Chimie*, tom. lxi. p. 713. This memoir gained the annual prize on galvanism given by the French Institute.

that

that the intervention of the substance employed does not modify in the least the electrical tension, which remains at the natural maximum at each pole; and that this substance belongs to the class of perfect insulators.

2. The two poles exercise, by the intermedium of the substance applied, a reciprocal reaction so intimate, that, neutralising itself perfectly, every phænomenon peculiar to each ceases, and we cannot act upon any of them in a distinct or appreciable manner. These substances belong to the class of perfect conductors.

3. The substance applied to the two poles permits their reciprocal reaction and closes the Galvanic circle, but in a manner so imperfect, that the distinct effect of each pole continues to manifest itself, and by the intermedium of this substance we may influence separately each pole in particular, according as we act with one or other of the imperfect conductors.

4. The substance by which we make the two poles communicate, acting like a perfect conductor, when we apply it separately to each of them, is found however to belong exclusively to the positive pole, the instant we apply it to both of them at once, with the view of closing the galvanic circle. The conductors of this class do not operate the closure of the circle, on account of the isolated state in which they leave the negative effect; and we cannot by their intermedium in the conflict of the two poles either charge the positive or discharge the negative.

5. Lastly, the effect previously indicated is reproduced in the same manner, but in an inverse sense; *i. e.* the substance which separately acts upon each pole, as a perfect conductor would do, belongs entirely to the negative pole from the moment we apply it simultaneously to the two extremities of the pile; hence results the maximum of electrical tension for the positive pole, and the impossibility of producing any divergency on the negative side by the intermedium of substances of this fifth class.

The phænomena of the three first classes are sufficiently well known: I shall therefore confine myself to a detail of the facts which demonstrate the existence of conductors of the

the 4th and 5th class. These facts add to the advantage of novelty, that of presenting problems which it would be interesting to resolve, and new views in Galvanic inquiries.

§ I.

Conductors, which in the Conflict of the two Poles isolate the negative Effect while they continue to propagate positive Electricity.

When we apply separately to each of the poles of the pile the flame of a lamp of spirits of wine, it acts like a perfect conductor; but when applied simultaneously to the two poles it totally isolates the negative effect, while it continues to conduct with the same energy the positive electricity, and consequently from this partial isolation the closure of the Galvanic circle does not take place.

If at either of the poles of a pile well isolated, of 100 couples or thereabouts, we apply an electrometer formed of gold leaves, they soon assume the degree of divergency which corresponds to the energy of the pile, and to the more or less perfect isolation by the ambient air of the opposite pole. When the instrument is stationary in its divergency, we present to the metallic wire of the opposite pole the flame of a spirit of wine lamp perfectly isolated; the electrometer does not increase in divergency; but the instant we put the flame in communication with the ground, by introducing into it a metallic wire not isolated, the electrometer assumes a divergence as strong as if we had made the isolation of the opposite pole to cease.

We may also take it radically away from each of them, each pole being in communication with an electrometer, by a metallic wire. If an isolated flame touches one of these wires, the electrometer with which it corresponds loses no part of its divergencey, but it is entirely taken away from it the instant we place the flame in direct communication with the ground.

We see these two effects united by preparing two flames perfectly isolated, and by conducting into each one of the metallic wires proceeding from the extremities of the pile,

pile. If the isolation is perfect, electrometers, when applied to the two points, will indicate in a few seconds the same state of divergency as if the poles were not in contact with the flame. If, however, we put one of the flames in communication with the ground, the electrometer of this pole immediately loses all its divergency, and it passes to the maximum in the electrometer of the opposite pole. The alternate contact of the two flames therefore produces the same effect as if we had immediately touched the extremities of the pile itself.

Lastly, in order to ascertain still more convincingly that flame is a better conductor for all the effects of the pile which are not connected with the closing of the circle, we may bend the metallic stalk which surmounts an electrometer so as to make the point of it touch an isolated flame; we must bring into this same flame a metallic wire coming from one of the poles; and then, if we touch the opposite pole, the electrometer will receive through the flame the maximum of the divergency relative to this case. If we afterwards touch the electrometer itself, the pole at which it communicates, by the intermedium of the flame, is discharged. At least upon touching the flame we discharge at one and the same time both the electrometer of this pole and that which enters into the flame.

Here there are electrical effects of the pile, which flame certainly does not isolate. Let us see what is the influence of this same flame, when we apply it simultaneously to the two poles, with the intention of closing by its intermedium the Galvanic circle.

After having fixed to a very sensible electrometer each pole of a pile well isolated and composed of about 150 couples of silver and zinc, we make to proceed from each pole a metallic wire supported by a stalk perfectly isolating: the extremities of the two wires are brought so near as to enter into the flame at one and the same time. Upon an isolating support we then place a spirit of wine lamp, and the experiment begins. The instant we put the two metallic wires in communication with the flame, so long as the latter remains isolated,

isolated, the electrometers of the two poles diverge nearly as if the two polar wires were perfectly isolated; and it is only after a certain time that the electrometer of the negative pole offers a divergence a little stronger than that of the positive: but as to the rest, every thing seems to bespeak an absolute isolation; for, on putting any one of the poles in communication with the ground, its electrometer loses all its divergency, and that of the opposite pole arrives at the maximum; and if we touch simultaneously the two poles, we receive a shock as strong as if the two poles were isolated by a body of air.

It would seem that hitherto this experiment alone has been adduced, as proving that flame isolates all the Galvanic effects; but we shall see, by the following facts, that this isolation is only partial, and that, as to the positive pole, flame continues to be an excellent conductor.

Every thing remaining as in the preceding experiment, we do away the isolation of the flame by introducing into it a metallic rod. Instantly all the divergence passes to the negative pole, and the positive is absolutely discharged. If we have previously given to the negative electrometer the strongest possible degree of divergence by touching the opposite pole, the application of a good conductor to the flame will never take away the least particle of this negative divergency, while this same application destroys in an instant every vestige of divergence given anteriorly to the positive pole, and carries it to the negative side in the strongest possible degree.

Flame, therefore, belongs entirely to the positive pole, because it is impossible to act upon the negative side by its means, and all the electro-metrical indications announce that the Galvanic circle is not closed by its interposition.

While it is an excellent conductor for each pole of the pile in particular, it isolates completely the negative side in the conflict of the two poles, without ceasing to be eminently conductory of the positive effect.

§ II.

§ II.

Of those Conductors which during the Conflict of the two Poles isolate the positive Effect, while they continue to propagate negative Electricity.

A solid prism of soap, completely dried and applied to any of the poles of the Galvanic pile, propagates into the ground all the electricity of this pole, and produces on the opposite side the maximum of electrical tension. There is not, relative to this effect, any difference between its action upon the two poles; and the soap acts as well as the most perfect conductor can do.

If we make a metallic wire proceed from each extremity of the pile, and if we plunge the free extremity of each of these two wires into the same prism of soap perfectly isolated, we do not perceive any remarkable effect; *i. e.* after having brought to the same level of tension the two poles, by applying to them an isolated metallic rod, the electrometers of the two poles will act as they did before the intervention of the soap, and when a body of air isolated them perfectly from each other.

But at the same instant we put the soap in free communication with the ground, the positive electrometer presents the maximum of divergency, and that of the negative side loses every vestige of it, precisely as if we had placed the negative pole itself in communication with the ground. Consequently, the soap which isolates the positive effect is a conductor perfectly continued for the negative effect to which it belongs in all its extent; for, if we touch it with a very small point, very near the point where the wire of the positive pole enters, it is entirely impossible to take away thereby any portion of positive electricity, so absolute is the isolation of this pole.

The flame of phosphorus also conducts the effect of each pole in particular, and in the conflicts of the two effects it belongs exclusively to the negative pole, like soap.

I have often witnessed the same property in gelatine brought to a certain degree of concentration, as well as in

ivory:

ivory: but other masses of these same substances presenting these phænomena in a very equivocal manner only, I abstain from deciding upon them, and confine myself to pointing out soap and the flame of the phosphorus as composing the fifth class of substances, which, *when applied to each individual pole, act as excellent conductors, but which, when interposed between one pole and the other, isolate the positive effect, without ceasing to conduct in a perfect manner the negative electricity.*

In order to describe precisely the whole of these phænomena by concise expressions which may fix them in the memory, I propose the following nomenclature, for avoiding circumlocutions, and to banish equivocal expressions.

The bodies which we may apply to the poles of a pile are:

Isolating (A), 1st class.

Conductors perfect (B), 2d class.

Ditto imperfect
- Bipolar (C), 3d class.
- Unipolar
  - Positive (D), 4th class.
  - Negative (E), 5th class.

A. *Isolating by their contact.*—These do not change any of the two poles separately, and do not take away the charge from any of them in the conflict of the two poles; they consequently isolate every effect at the two poles at once. Such are, *glass, roots, water, water in a state of vapour, sulphur and its flame, amber but not its flame, &c.*

B. *Perfect conductors.*—They charge and discharge each individual pole; but in the conflict of the two poles every vestige of polarity disappears at the positive as at the negative, and the circle is perfectly closed. *To this class belong the metals, and all of them in the same degree.*

C. *Imperfect conductors.*—The characteristic effects of the two poles may also be discerned during the application of these substances to the two poles simultaneously. They are divided into three classes.

Bipolar.—They close the Galvanic circle, but they present in their length two stripes opposed to each other by their electrical effects. *Water in a fluid state, and such bodies as are impregnated with it.*

D. *Positive*

D. *Positive unipolar.*—The Galvanic circle is not closed; the substance applied to the two poles, only conducts the positive effect and isolates the negative; whence results the charge of the negative, and the impossibility of charging the positive.

This faculty belongs to the flame of hydrogen gas, and to that of the hydrocarbonated bodies.

E. *Negative unipolar.*—The Galvanic circle is not closed; the substance applied to the two poles isolates the positive effect and conducts the negative effects. Its contact charges the positive exclusively, and adds nothing to the negative. This class is composed of *the flame of phosphorus* and of *the alkaline soaps.*

XLII. *On the Musical Temperament of Keyed Instruments.* *By* William Hawkes, *Esq.*

To Mr. Tilloch.

SIR,

I observed in the Monthly Magazine of September 1806, some critical remarks on earl Stanhope's Method of tuning Keyed Instruments; I have also seen Mr. Farey's calculations and his philosophical disquisitions in your Magazines of April and May last on the subject; but they are not sufficiently satisfactory to induce me to alter my opinion of the method I published in 1805, prior to earl Stanhope's. I think the principle upon which the noble earl has acted is very ingenious, and I sincerely wish his lordship had succeeded in his endeavour to give a better effect to the instrument by his means of dividing the enharmonic difference of those tones which in extended modulation (from the imperfection of the instrument) become substitutes for real ones, and which inevitably produce those disgusting chords termed *wolves* by some. It is to be lamented there are such insurmountable difficulties in melodizing or softening those tones; indeed, I despair of ever hearing tolerable harmony throughout the twenty-four keys of the present scale; consequently, conceive it to be impracticable, unless two scales, the

the one with flats, the other with sharps, be adopted. The method that I chose in my publication on temperament, 1805, (with submission to the learned Dr. Bradley, whom I have not the honour of knowing) was upon the principle of a specific division of the major tones of the scale, which must occasionally serve as minor tones, and is effected by subtracting one-fifth of a comma from a certain number of perfect fifths, in order to temper those tones which in every additional flat or sharp consequent to a new key require a tone a comma higher or lower; and to tune perfect, or in a small degree sharper, some of the fifths to meet such tones as are substitutes for real ones. This seems to me to be the best principle for tempering our present scale; and any attempt to reduce the anomalous tone B ✻, the result of the twelfth quint, or fifth, is much easier understood than accomplished. To some theorists it is known, that by tuning all the fifths one-eleventh part of a comma flat with respect to that tone, it will be reduced so near to the original, C, as to be an imperceptible difference to the ear. But what then will become of the major thirds, and their replicates? The answer is ready: not one of them will be tolerably in tune; every one of them nearly a comma too sharp: *shocking to a musical ear*. Chords, too, that most frequently occur in duets and trios, the semi-tones major will be also nearly a comma too flat, and all the harmonics disgusting.

With respect to Mr. Farey's observations on modulation, I can only say, that, since abrupt cadences are so frequent in modern music, it would be difficult to ascertain by any calculation what keys are more or less used by the moderns; consequently, their times of occurrence cannot, with any degree of certainty, be known. Modulation often extending to the 7th sharp and 7th flat, I much wish to know what temperament can reach the one or the other, without producing a worse scale than the present *; for the power (or refutation of the fifth) which reduces the excess of one (viz. the 12th quint), gives nearly a two-fold excess to the other

* Does the author here mean his own system, or what particular system does he mean?—Edit.

other (namely, the major third). My allusion is to the 12th quints and the major thirds compared with the octave; that being the case, every attempt must be unavailing to temper or assimilate two tones in direct opposition to each other. I am therefore fully persuaded that the method I have acted upon is good, being nearly the same in its effect as that now in general practice; and unless an instrument can be furnished with two scales, one with sharps and another with flats, there are very little hopes of ever meliorating common keyed instruments: but I am of opinion that such an instrument may be constructed without an additional key; of which more hereafter. The temperament would then be by flattening all the fifths of each scale to the 7th sharp and 7th flat; by which method the enharmonics, or quarter-tones difference, will cease to be, and no other substitutes but major and minor tones for each other will be required; for that tone which in one key is a major, will in another be apposite for a minor tone, by the above mode of temperament.

I am, yours, &c.

WILLIAM HAWKES.

XLIII. *On the Discovery of the Fluoric Acid in the Enamel of Human Teeth; on Tantalite and Yttro-tantalite; on Cerium; and on the Production of the Muriatic Acid by Means of Galvanism. Extracted from a Letter of M.* BERZELIUS *to M.* VAUQUELIN*.

Stockholm, Nov. 2, 1806.

HAVING been informed of the discovery of M. Morichini, who found fluoric acid in the enamel of teeth †, I made an analysis of bones a little more in detail than any hitherto attempted. I found that fresh bones contain, as well as the enamel of teeth, fluate of lime, and that this fluate, by absorption from the bones, exists also in urine, dissolved with phosphate of lime by means of free phosphoric acid. The

* *Annales de Chimie*, tom. lxi. p. 256.

† See Phil. Mag. vol. xxvii. p. 88.

precipitate

precipitate obtained from urine by means of lime water, when washed and dried, gives with sulphuric acid fluoric acid gas, which corrodes glass. But there must be a considerable quantity of this precipitate, in order to have very sensible marks of it. On decomposing, by the acetate of lead, burnt human bones dissolved in nitric acid, I found magnesia in them, although in a smaller quantity than in the bones of herbivorous animals.

The following is the result of my labours:

	Dried Human Bones.	Enamel of Human Teeth.	Bones of Oxen.	Enamel of Oxen Teeth.
Cartilage - -	32·17	—	33·30	3·56
Blood-vessels -	1·13	—		
Fluate of lime -	2·00	3·2	9·30	4·00
Phosphate of lime -	51·04	85·3	55·85	81·00
Carbonate of lime -	11·30	8·0	3·85	7·10
Phosphate of magesia	1·16	1·5	2·05	3·00
Soda, muriate of soda, water, &c.	1·20	2·0	2·45	1·34
	100	100	100	100

Human teeth contain the same earthy substances as enamel does; but they contain cartilage also. The Analysis of Urine above alluded to, was printed at the beginning of the year in a kind of Journal of Chemistry, edited by Kisinger and myself, under the title of *Afhandlingari Jysik, Kemi af Mineralogi*, of which we should have had the honour of transmitting a copy, if the war had not interrupted all communication.

M. Gahn has found tantalite and yttro-tantalite at Tahlun. By his address in managing the blowpipe he discovered that the tantalum of M. Eckeberg is only tin combined with an earth which he has not yet ascertained. M. Eckeberg is at present occupied with a more detailed examination of this combination.

The Upsal chemists at first thought that cerium was only a mixture of barytes, yttria, and magnesia; M. Eckeberg, wishing to compare them, found that yttria, when a long time exposed to the fire, gives also oxymuriatic acid by dis-

solving it in muriatic acid. Might the latter not be one of these metals, changed in its nature, as it were, like uranium, titanum, and cerium? M. Gahn sent me mixtures of cerium and of iron, partly metallic and partly forming a grayish powder.

The production of muriatic acid and of soda, by means of the Galvanic pile, in perfectly pure water, has not succeeded among us, although our piles are proper enough in point of the size of the metallic plates, and the number of the pairs. Pure water is a very bad conductor; the pile can hardly be made to act with it; but if there is the least particle of any salt, the decomposition is more rapid, and the salt is decomposed; the positive wire seizing the acid, and the negative the alkaline, base. It seems probable to me that the gentlemen who thus compose the muriatic acid are led into an error by the decomposition of muriate of soda, contained in their water, perhaps badly distilled.

XLIV. *Thirty-fifth Communication from Dr.* Thornton, *relative to Pneumatic Medicine.*

Cure of Typhus Fever by Yeast.

DEAR SIR,

In sending you the detail of the case of my sister, whose extraordinary recovery from the last stage of a violent typhus fever reflects the highest honour on your skill and decision, I am induced particularly to call your attention to the instantaneous salutary effects which arose from the exhibition of yeast. The use of yeast in medicine has not, I believe, till very lately been admitted into practice in this country; it is still by many looked upon as of dubious utility, and by some is considered as wholly useless, or partially noxious. It is not only from this case, but from several others that I have witnessed, that I am convinced of its highly beneficial effects; and I have seen it successfully used in several places in my native country, Holland. It was, as near as I can recollect, in the year 1799, that I first knew it exhibited.

This

This was at Rotterdam, and occurred in a case of confluent small-pox, the virulence of the fever attending which was effectually and quickly subdued by this simple antiseptic. I mention this particularly because it has been held to be a very late discovery, and the honour of it has been ascribed to an ingenious clergyman * of this country, which I am anxious should be bestowed where it is most due, upon the practitioners of my native place. It may not be irrelevant here to remark, that yeast is made up and preserved in Holland in almost a solid form, being of the consistence of putty, in which state it will keep, if the weather be moderately cool, for six weeks or longer; and it is sent in bladders from one place to another, so that no part of the country, or even a remote farm-house, need ever be in want of this necessary article. I know not the mode adopted to reduce the yeast to this consistency, but I think it would be worth the trouble of inquiry, and adoption in this country, where much inconvenience is sometimes felt from the liquid and diffused state in which alone yeast is met with, and its great liability to be spoiled.

The simplicity and cheapness of this medicine, whilst it is a recommendation to the intelligent physician, is an obstacle to its introduction where apothecaries prescribe. What can be more simple than the happy discovery of the effects of cold effusion? or what more widely salutary than the inestimable discovery of vaccination, on which you, sir, have so ably exercised your pen? And yet we see that, from interested or other motives, these simple but effectual remedies are scouted by a considerable number of practitioners.

In addition to Mrs. W.'s case, whom you personally attended, I have to add the case of her youngest child, 18 months

* Mr. Cartwright, whose claim to the merit of the discovery is rather confirmed, than set aside, by M. du Moulin's own statement. To the best of his recollection it was in the year 1799 that he first knew yeast exhibited as a medicine in Holland; but Mr. Cartwright's discovery was some years earlier. I cannot at the present moment lay my hands on any document ascertaining the precise date, but I find it referred to in Dr. Beddoes's *Considerations on Factitious Air*, part iii. published in 1795, p. 61. This honour of the discovery must therefore still remain with Mr. Cartwright.—Edit.

 old,

old, under my own care. This little girl, having caught the typhus fever of her mother, is now in a state of convalescence in consequence of the administration of yeast, by which, in less than the usual period, the fever has been entirely overcome.

I shall be happy if this communication prove any way interesting to the philosophic world. And remain,

Dear sir,

Your most obedient servant,

J. C. Du Moulin.

Arnold's Place,
Newington Butts,
July 18, 1807.

Dr. Thornton.

Observations on these Cases by Dr. Thornton.

1. Lately, a relation of Mr. Sutherland, the engraver, who was at the point of death in typhus fever, experienced a very unexpected change, from my ordering the administration of *yeast*.

2. In addition, in this case, I ordered water impregnated with *fixed air*, which was given with some port wine, and proved very efficacious.

3. The yeast being from the baker's, the bitter principle is much washed out by means of water; and I usually order a table-spoonful every three hours, and vegetable food in the interval, with porter.

4. The bowels must be kept open with a few grains of calomel at first, and afterwards each evening, unless relieved in the course of the day.

5. Bark and serpentaria, a scruple of the former to half a scruple of the latter, may be usefully added to the yeast; but I have been equally successful in numberless cases without any such addition.

6. The rationale of the operation of yeast from the *fixed air* it contains, has been before explained.

XLV. *On*

XLV. *On the Alkalies of Commerce, and on the least expensive Process for ascertaining their commercial Value by Means of the Instrument called the Alkali-meter. Read to the Academy of Rouen, 5 Thermidor, An. 13, by M.* Descroizilles *senior.*

[Concluded from p. 252.]

Necessity of a sufficient Proportion of Water, with supersaturated Carbonate of Potash, in order that the Potash may be completely disengaged by Means of Lime.

It must have been observed that, in the first article quoted in the preceding section, it has been prescribed to mix the potash with *double* its weight of lime, while in the second article no more than *one-half* is demanded; it is true, that less is required for the truly neutral carbonate of potash, because it contains a less quantity of alkali, and because the water of crystallization is here in greater proportion than in the supersaturated carbonate of potash; but if we consider that the richest in alkali (being that which is obtained from cream of tartar) absorbs only 0·72 of its weight in sulphuric acid, while the quantity absorbed by the carbonate is 0·50, we must admit that the proportions indicated in the two cases are no longer relative to each other.

On the other hand, numerous experiments, made on a large scale, have constantly proved to me that four-tenths of lime are sufficient to take off the carbonic acid from the best potash of commerce; How happens it, therefore, that twenty tenths have been prescribed, and still fears entertained that the potash was not perfectly caustic? I shall now show that this fear appears well founded, so long as the following observation of mine is unknown: I have ascertained that, whatever be the proportion of the lime to the supersaturated carbonate of potash, if the proportion of water to this salt is not as seven to one, there will be a quantity of carbonate not decomposed, and proportionate to the deficit of water, in the seven parts which are strictly necessary. Besides, if we consult the tables of Bergman as to the re-

spective quantities of carbonic acid which are found in the supersaturated carbonate of potash and in the carbonate of lime, we shall see that four-tenths of lime must be always sufficient for taking up all the carbonic acid contained in any given quantity of the best potash of commerce.

But here is a double experiment, which we may verify: Take of quicklime, 4 parts; of white potash of Russia, 10 parts; pure water, 70 parts: slake the lime with a portion of the water, and dissolve the potash in the remainder: afterwards mix the whole, boil it for a few minutes, then allow it to cool, and restore the whole of the 24 parts, which may have been diminished by the slaking of the lime and the ebullition; mix the added water very minutely; allow the whole to subside, and decant a little of the liquor: you will find, upon trying it, that no precipitate is formed in the lime water, and that it makes no efflorescence with the acids.

In the second place, take the same proportions of lime and potash, but add only 50 parts of water to it; then try the liquor which will result from this, and you will find that it occasions a precipitate in lime water and an effervescence with the acids. Finally, add 20 additional parts of water to the first 50, and mix the whole carefully: the liquor, when decanted, will occasion neither precipitate nor effervescence: in a word, the alkali will have become caustic in consequence of the extraction of all its carbonic acid.

I am inclined to think that this observation may be usefully applied to the caustification of soda.

Explanation of the Causes of the Uncertainty in the Processes of Caustification.

We now see clearly the reason why we cannot obtain a perfectly caustic alkali, if we do not add a sufficient quantity of water to the mixture of lime and supersaturated carbonate of potash. Although it might be thought we have used too much lime already, yet we must put still more into the mixture; but as it might become too thick, we are under the necessity of adding a new portion of water, which alone completes the caustification, although this has been, nevertheless, attributed to the new addition of lime.

Lastly,

Lastly, we see the reason why from a mixture of lime and of supersaturated carbonate of potash, with too little water, the first lixivium, which may be filtered, will be effervescent, while that which may be obtained from washings, *i. e.* by the addition of a new quantity of water, will come off entirely caustic.

These alternatives might take place upon the execution of the second of the processes, already quoted, of M. Fourcroy, because the proportion of water is then indeterminate; while in the first process, if it be equal to eight or ten times the total weight of the supersaturated carbonate of potash and lime, it is evident that we ought consequently to obtain potash totally deprived of carbonic acid, if we have never, by the ebullition prescribed of two or three hours, reduced the proportion of water to a less quantity than seven times the weight of the supersaturated carbonate of potash: for my own part, I am convinced that an ebullition of a few minutes is at all times sufficient.

Potash exists in all Lime which is burnt by a Wood Fire.

I shall now proceed to announce some facts connected with the preceding obsérvations.

So long ago as the year 1795, my brother and I observed that lime, when burnt by means of wood, contained potash: this will not appear surprising when we consider the great lightness of some particles of ashes, which may be called *flowers of ashes:* there is no wood fire, let it be ever so small or moderate, that does not emit these flowers, and we see them attached to the kitchen utensils which are heated upon a wood fire. Their alkaline property will be evident if we first moisten the tip of the finger with a little saliva, and afterwards lay it gently upon the vessels to which these flowers are attached. On afterwards applying the finger to the tongue, a very decided alkaline taste will be perceived. It may be easily conceived, therefore, that in a violent fire, such as that of a lime-kiln, the current of air which traverses it carries these light ashes through the pieces of calcareous stones. The earthy part of these ashes stops at the exterior of

of the pieces of limestone, which contract a colour more or less gray; while the potash going really into vapours, from the violence of the fire, passes into the interior of the stone, which, after being burnt, remains very white. We carefully detached and analysed a great quantity of this external matter, and we there found the exact proportion of potash which existed in the whole mass of the pieces: this proportion was nearly a 5-100dth of the weight of the lime.

Chemists who may repeat our experiments will find in lime more or less potash than the quantity above announced; it must vary according to circumstances, and particularly from the nature of the wood employed in burning the lime.

Some chemical Anomalies clearly explained by the Presence of Potash in Lime, and the Necessity of previously washing the Lime, or of employing, in chemical Experiments and medical Preparations, Lime burnt with Pit Coal.

By this means we may consequently explain the pretended decomposition of a very small quantity of muriate of soda operated by a very great quantity of lime; an error into which the immortal Scheele has fallen, and in which he has been followed by some of our most celebrated French chemists *.

Some chemists have also been led into error upon mixing sulphate of soda and lime.

This also explains the disputes among the chemists of the Academy of Sciences (and which are contained in the memoirs of that learned body about the commencement of the last century) upon a salt which some of them said was found in lime, while others denied its existence entirely.

Thence proceeds the well-founded idea of a property peculiar to the first water drawn off lime; a property which has been improperly contested, and which cannot exist in second lime water.

* I do not mean, however, to deny that in some particular cases lime can decompose the muriate and the sulphate of soda; I merely insist that we may be allowed to doubt it, provided we are not previously certain that the lime mixed with the one or other of these salts did not contain potash.

This

This is the reason why some sugar-refiners take great pains, and expend a great deal of money, for a first lime water, which they might obtain by a little potash and lime.

Lime water is recommended by some oculists: it is for them to consider whether it is proper that it should contain caustic potash, *i. e.* the caustic stone. This observation is equally applicable to the internal use of lime water, prescribed by some physicians.

In chemical experiments, therefore, such as making the precipitate and sulphuret of lime, &c., we must employ lime burnt with coal, or it must be washed if it has been burnt with wood.

Origin of Natural Soda, as it is called.

The efflorescences of carbonate of soda, found under the arches of the Pont-neuf, at Paris; those of the cellars at Dieppe and other maritime places, are no longer objects of astonishment. A few years ago, when at Dieppe, I had occasion to ascertain the origin of these efflorescences mixed with sulphate of soda. I descended into the cellar, under the kitchen, of my paternal residence, where in my infancy I had seen the linen of a numerous family washed. The arch of this cellar had been formerly plastered. The greatest part of this plaster was now detached; but where it still remained there was sulphate of soda, while beside it there was only carbonate of soda. The origin of these two salts was no longer a mystery to me; for, independently of the potash existing in the lime of the vault, the leys, when the linen was washed, had penetrated the masonry; they had there met with kitchen salt, which had been consequently decomposed; hence the carbonate of soda. This last, wherever it found sulphate of lime, had given place to the formation of sulphate of soda.

It often requires but a slight degree of reflection, upon all the circumstances which accompany results most extraordinary in appearance, in order to explain the most striking contrasts; and for this purpose a great number of facts, hitherto ill explained, must be reviewed.

First

First Indication of the Mineralogical Inquiries necessary to be made as to the Existence of Potash.

In an essay upon the art of making saltpetre, which I presented to the National Institute several years ago, I explained the origin of what is called *natural soda,* and I took that occasion to inquire *what became of the potash confined in the bowels of the earth.* About a year afterwards M. Klaproth discovered it in several volcanic productions; and his discovery was soon confirmed by M. Vauquelin. It is peculiarly gratifying to me to have suggested the idea of these useful researches.

Probable Origin of Natrum.

I am strongly inclined to think that the natrum of the lakes in Egypt and other places owes its origin to the decomposition of the muriate of soda by potash, which is one of the products of the disorganization of vegetables; for the shores of the six lakes of Egypt, and the adjacent soils, are covered with jonquils and roses, which must annually furnish plenty of potash. It would be easy to verify this explanation by experiments, which promise more success than those which have been made with other views. To conclude: the explanation which I thus gave, previous to the late memorable travels of several of our learned men in Egypt, before it be confirmed, would require us to prove the coexistence of the muriate of potash along with the other salts, in the lakes of natrum and in the waters of the Nile

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XLVI. *Report of the Royal College of Physicians of London on Vaccination. Printed by Order of the House of Commons, dated* 8*th July* 1807.

The Royal College of Physicians of London, having received his majesty's commands, in compliance with an address from the house of commons "to inquire into the state of vaccine inoculation in the united kingdom, to report their opinion

opinion and observations upon that practice, upon the evidence which has been adduced in its support, and upon the causes which have hitherto retarded its general adoption," have applied themselves diligently to the business referred to them.

Deeply impressed with the importance of an inquiry which equally involves the lives of individuals and the public prosperity, they have made every exertion to investigate the subject fully and impartially. In aid of the knowledge and experience of the members of their own body, they have applied separately to each of the licentiates of the college; they have corresponded with the Colleges of Physicians of Dublin and Edinburgh; with the Colleges of Surgeons of London, Edinburgh, and Dublin; they have called upon the societies established for vaccination, for an account of their practice, to what extent it has been carried on, and what has been the result of their experience; and they have, by public notice, invited individuals to contribute whatever information they had severally collected. They have in consequence been furnished with a mass of evidence, communicated with the greatest readiness and candour, which enables them to speak with confidence upon all the principal points referred to them.

I. During eight years, which have elapsed since Dr. Jenner made his discovery public, the progress of vaccination has been rapid, not only in all parts of the united kingdom, but in every quarter of the civilized world. In the British islands some hundred thousands have been vaccinated; in our possessions in the East Indies upwards of 800,000; and among the nations of Europe the practice has become general. Professional men have submitted it to the fairest trials, and the public have, for the most part, received it without prejudice. A few, indeed, have stood forth the adversaries of vaccination on the same grounds as their predecessors who opposed the inoculation for the small-pox, falsely led by hypothetical reasoning in the investigation of a subject which must be supported or rejected upon facts and observation only. With these few exceptions, the testimony in favour of vaccination has been most strong and satisfactory; and the practice

practice of it, though it has received a check in some quarters, appears still to be upon the increase in most parts of the united kingdom.

II. The College of Physicians, in giving their observations and opinions on the practice of vaccination, think it right to premise, that they advance nothing but what is supported by the multiplied and unequivocal evidence which has been brought before them, and they have not considered any facts as proved but what have been stated from actual observation.

Vaccination appears to be in general perfectly safe; the instances to the contrary being extremely rare. The disease excited by it is slight, and seldom prevents those under it from following their ordinary occupations. It has been communicated with safety to pregnant women, to children during dentition, and in their earliest infancy; in all which respects it possesses material advantages over inoculation for the small-pox; which, though productive of a disease generally mild, yet sometimes occasions alarming symptoms, and is in a few cases fatal.

The security derived from vaccination against the small-pox, if not absolutely perfect, is as nearly so as can perhaps be expected from any human discovery; for amongst several hundred thousand cases, with the results of which the college have been made acquainted, the number of alleged failures has been surprisingly small, so much so, as to form certainly no reasonable objection to the general adoption of vaccination; for it appears that there are not nearly so many failures, in a given number of vaccinated persons, as there are deaths in an equal number of persons inoculated for the small-pox. Nothing can more clearly demonstrate the superiority of vaccination over the inoculation of the small-pox, than this consideration; and it is a most important fact, which has been confirmed in the course of this inquiry, that in almost every case where the small-pox has succeeded vaccination, whether by inoculation or by casual infection, the disease has varied much from its ordinary course; it has neither been the same in the violence nor in the duration or its symptoms, but has, with very few exceptions, been remarkably

remarkably mild, as if the small-pox had been deprived, by the previous vaccine disease, of all its usual malignity.

The testimonies before the College of Physicians are very decided in declaring, that vaccination does less mischief to the constitution, and less frequently gives rise to other diseases, than the small-pox, either natural or inoculated.

The college feel themselves called upon to state this strongly, because it has been objected to vaccination, that it produces new, unheard-of, and monstrous diseases. Of such assertions no proofs have been produced, and, after diligent inquiry, the college believe them to have been either the inventions of designing, or the mistakes of ignorant, men. In these respects then, in its mildness, its safety, and its consequences, the individual may look for the peculiar advantages of vaccination. The benefits which flow from it to society are infinitely more considerable; it spreads no infection, and can be communicated only by inoculation. It is from a consideration of the pernicious effects of the small-pox, that the real value of vaccination is to be estimated. The natural small-pox has been supposed to destroy a sixth part of all whom it attacks; and that even by inoculation, where that has been general in parishes and towns, about one in 300 has usually died. It is not sufficiently known, or not adverted to, that nearly one-tenth, some years more than one-tenth, of the whole mortality in London, is occasioned by the small-pox; and however beneficial the inoculation of the small-pox may have been to individuals, it appears to have kept up a constant source of contagion, which has been the means of increasing the number of deaths by what is called the natural disease. It cannot be doubted that this mischief has been extended by the inconsiderate manner in which great numbers of persons, even since the introduction of vaccination, are still every year inoculated with the small-pox, and afterwards required to attend two or three times a week at the places of inoculation, through every stage of their illness.

From this, then, the public are to expect the great and uncontroverted superiority of vaccination, that it communicates

nicates no casual infection, and, while it is a protection to the individual, it is not prejudicial to the public.

III. The College of Physicians, in reporting their observations and opinions on the evidence adduced in support of vaccination, feel themselves authorized to state, that a body of evidence so large, so temperate, and so consistent, was perhaps never before collected upon any medical question. A discovery so novel, and to which there was nothing analogous known in nature, though resting on the experimental observations of the inventor, was at first received with diffidence: it was not, however, difficult for others to repeat his experiments, by which the truth of his observations was confirmed, and the doubts of the cautious were gradually dispelled by extensive experience. At the commencement of the practice, almost all that were vaccinated were afterwards submitted to the inoculation of the small-pox; many underwent this operation a second, and even a third time, and the uniform success of these trials quickly bred confidence in the new discovery. But the evidence of the security derived from vaccination against the small-pox does not rest alone upon those who afterwards underwent variolous inoculation, although amounting to many thousands; for it appears, from numerous observations communicated to the college, that those who have been vaccinated are equally secure against the contagion of epidemic small-pox. Towns indeed, and districts of the country, in which vaccination had been general, have afterwards had the small-pox prevalent on all sides of them without suffering from the contagion. There are also in the evidence a few examples of epidemic small-pox having been subdued by a general vaccination. It will not, therefore, appear extraordinary, that many who have communicated their observations should state, that though at first they thought unfavourably of the practice, experience had now removed all their doubts.

It has been already mentioned that the evidence is not universally favourable, although it is in truth nearly so; for there are a few who entertain sentiments differing widely from those of the great majority of their brethren. The

college,

college, therefore, deemed it their duty, in a particular manner, to inquire upon what grounds and evidence the opposers of vaccination rested their opinions. From personal examination, as well as from their writings, they endeavoured to learn the full extent and weight of their objections. They found them without experience in vaccination, supporting their opinions by hearsay information and hypothetical reasoning, and, upon investigating the facts which they advanced, they found them to be either misapprehended or misrepresented; or that they fell under the description of cases of imperfect small-pox, before noticed, and which the college have endeavoured fairly to appreciate.

The practice of vaccination is but of eight years standing, and its promoters, as well as opponents, must keep in mind, that a period so short is too limited to ascertain every point, or to bring the art to that perfection of which it may be capable. The truth of this will readily be admitted by those acquainted with the history of inoculation for the small-pox. Vaccination is now, however, well understood, and its character accurately described. Some deviations from the usual course have occasionally occurred, which the author of the practice has called spurious cow-pox, by which the public have been misled, as if there were a true and a false cow-pox; but it appears that nothing more was meant than to express irregularity or difference from that common form and progress of the vaccine pustule from which its efficacy is inferred. Those who perform vaccination ought therefore to be well instructed, and should have watched with the greatest care the regular progress of the pustule, and learnt the most proper time for taking the matter. There is little doubt that some of the failures are to be imputed to the inexperience of the early vaccinators, and it is not unreasonable to expect that further observations will yet suggest many improvements that will reduce the number of anomalous cases, and furnish the means of determining, with greater precision, when the vaccine disease has been effectually received.

Though the College of Physicians have confined themselves in estimating the evidence to such facts as have oc-

curred in their own country, because the accuracy of them could best be ascertained, they cannot be insensible to the confirmation these receive from the reports of the successful introduction of vaccination, not only into every part of Europe, but throughout the vast continents of Asia and America.

IV. Several causes have had a partial operation in retarding the general adoption of vaccination; some writers have greatly undervalued the security it affords, while others have considered it to be of a temporary nature only; but if any reliance is to be placed on the statements which have been laid before the college, its power of protecting the human body from the small-pox, though not perfect indeed, is abundantly sufficient to recommend it to the prudent and dispassionate, especially as the small-pox, in the few instances where it has subsequently occurred, has been generally mild and transient. The opinion that vaccination affords but a temporary security, is supported by no analogy in nature, nor by the facts which have hitherto occurred. Although the experience of vaccine inoculation be only of a few years, yet the same disease, contracted by the milkers of cows, in some districts has been long enough known to ascertain that in them, at least, the unsusceptibility of the small-pox contagion does not wear out by time. Another cause is, the charge against vaccination of producing various new diseases of frightful and monstrous appearance.

Representations of some of these have been exhibited in prints in a way to alarm the feelings of parents, and to infuse dread and apprehension into the minds of the uninformed. Publications with such representations have been widely circulated; and though they originate either in gross ignorance or wilful misrepresentation, yet have they lessened the confidence of many, particularly of the lower classes, in vaccination: no permanent effects, however, in retarding the progress of vaccination need be apprehended from such causes, for, as soon as the public shall view them coolly and without surprise, they will excite contempt and not fear.

Though the College of Physicians are of opinion that the progress of vaccination has been retarded in a few places by

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the above causes, yet they conceive that its general adoption has been prevented by causes far more powerful, and of a nature wholly different. The lower orders of society can hardly be induced to adopt precautions against evils which may be at a distance; nor can it be expected from them, if these precautions are attended with expense. Unless, therefore, from the immediate dread of epidemic small-pox, neither vaccination nor inoculation appear at any time to have been general, and when the cause of terror has passed by, the public have relapsed again into a state of indifference and apathy, and the salutary practice has come to a stand. It is not easy to suggest a remedy for an evil so deeply imprinted in human nature. To inform and instruct the public mind may do much, and it will probably be found that the progress of vaccination in different parts of the united kingdom will be in proportion to that instruction. Were encouragement given to vaccination, by offering it to the poorer classes without expense, there is little doubt but it would in time supersede the inoculation for the small-pox, and thereby various sources of variolous infection would be cut off; but till vaccination becomes general it will be impossible to prevent the constant recurrence of the natural small-pox by means of those who are inoculated, except it should appear proper to the legislature to adopt, in its wisdom, some measure by which those who still, from terror or prejudice, prefer the small-pox to the vaccine disease, may, in thus consulting the gratification of their own feelings, be prevented from doing mischief to their neighbours.

From the whole of the above considerations the College of Physicians feel it their duty strongly to recommend the practice of vaccination. They have been led to this conclusion by no preconceived opinion, but by the most unbiassed judgment, formed from an irresistible weight of evidence which has been laid before them. For when the number, the respectability, the disinterestedness, and the extensive experience of its advocates, is compared with the feeble and imperfect testimonies of its few opposers; and when it is considered that many who were once adverse to vaccination, have been convinced by further trials, and are

now to be ranked among its warmest supporters, the truth seems to be established as firmly as the nature of such a question admits; so that the College of Physicians conceive the public may reasonably look forward, with some degree of hope, to the time when all opposition shall cease, and the general concurrence of mankind shall at length be able to put an end to the ravages at least, if not to the existence, of the small-pox.

LUCAS PEPYS, President.

Royal College of Physicians,
10th April 1807.

James Hervey, Register.

XLVII. *Report of the King and Queen's College of Physicians in Ireland on Vaccination*.*

Dublin, Nov. 11, 1806.

THE practice of vaccination was introduced into this city about the beginning of the year 1801, and appears to have made inconsiderable progress at first. A variety of causes operated to retard its general adoption, amongst which the novelty of the practice, and the extraordinary effects attributed to vaccination, would naturally take the lead.

Variolous inoculation had been long, almost exclusively, in the hands of a particular branch of the profession, whose prejudices and interests were strongly opposed to the new practice; and by their being the usual medical attendants in families, and especially employed in the diseases of children, their opinions had greater effect upon the minds of parents. The small-pox is rendered a much less formidable disease in this country by the frequency of inoculation for it, than it is in other parts of his majesty's dominions, where prejudices against inoculation have prevailed; hence parents, not unnaturally, objected to the introduction of a new disease, rather than not recur to that, with the mildness and safety of which they were well acquainted.

* From Report on Vaccination, printed by order of the House of Commons, dated 8th July 1807.

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In the beginning of the year 1804, the cow-pox institution was established under the patronage of the earl of Hardwicke, and it is from this period that we may date the general introduction of vaccination into this city, and throughout all parts of Ireland.

The success of the institution in forwarding the new practice is to be attributed, in a great measure, to the respectability of the gentlemen who superintend it, and to the diligence, zeal, and attention of Dr. Labatt, their secretary and inoculator. In order to show the progress which has been made in extending vaccination, your committee refer to the reports of the Cow-pox Institution for the last two years, and to extracts from their register for the present year.

	Patients inoculated.	Packets issued to Practitioners in general.	Packets to Army Surgeons.
1804	578	776	236
1805	1,032	1,124	178
1806	1,356	1,340	220
Total -	- 2,966	3,240	634

In the above statement the numbers are averaged to the end of the present year, on the supposition of patients resorting to the institution as usual. The correspondence of the institution appears to be very general throughout every part of Ireland, and by the accounts received, as well from medical practitioners as others, the success of vaccination seems to be uniform and effectual. At the present period, in the opinion of your committee, there are few individuals in any branch of the profession who oppose the practice of vaccination in this part of his majesty's dominions.

It is the opinion of your committee that the practice of cow-pox inoculation is safe, and that it fully answers all the purposes that have been intended by its introduction. At the same time, your committee is willing to allow that doubtful cases have been reported to them as having occurred of persons suffering from small-pox who had been previously vaccinated. Upon minute investigation, however,

ever, it has been found that these supposed instances originated generally in error, misrepresentation, or the difficulty of discriminating between small-pox and other eruptions, no case having come to the knowledge of your committee, duly authenticated by respectable and competent judges, of genuine small-pox succeeding the regular vaccine disease.

The practice of vaccination becomes every day more extended; and, when it is considered that the period at which it came into general use in Ireland is to be reckoned from so late a date, your committee is of opinion that it has made already as rapid a progress as could be expected.

(Signed) James Cleghorn,
Daniel Mills,
Hugh Ferguson.

XLVIII. *Report of the Royal College of Physicians of Edinburgh on Vaccination**.

Physicians Hall, Edinburgh,
November 26, 1806.

Gentlemen,

The Royal College of Physicians of Edinburgh have but little opportunity themselves of making observations on vaccination, as that practice is entirely conducted by surgeon apothecaries, and other medical practitioners not of their college, and as the effects produced by it are so inconsiderable and slight, that the aid of a physician is never required.

The college know that in Edinburgh it is universally approved of by the profession, and by the higher and middle ranks of the community, and that it has been much more generally adopted by the lower orders of the people than ever the inoculation for small-pox was, and they believe the same to obtain all over Scotland.

With regard to any causes which have hitherto prevented its general adoption, they are acquainted with none, except the negligence or ignorance of parents among the common people, or their mistaken ideas of the impropriety or criminality of being accessary to the production of any disease

* From the House of Commons printed Report of 8th July 1807.

among

among their children, or the difficulty or impossibility, in some of our country districts, of procuring vaccine matter, or a proper person to inoculate.

The evidence in favour of vaccination appeared to the Royal College of Physicians of Edinburgh so strong and decisive, that in May last they spontaneously and unanimously elected Dr. Jenner an honorary fellow of their college,—a mark of distinction which they very rarely confer, and which they confine almost exclusively to foreign physicians of the first eminence.

They did this with a view to publish their opinion with regard to vaccination, and in testimony of their conviction of the immense benefits which have been, and which will in future be derived to the world, from inoculation for the cow-pox, and as a mark of their sense of Dr. Jenner's very great merits and ability in introducing and promoting this invaluable practice.

I have the honour to be,
Gentlemen,
Your most obedient humble servant,
TH. SPENS, C. R. M. Ed. Pr.

To the Royal College of Physicians of London.

XLIX. *Report of the Royal College of Surgeons of London on Vaccination**.

March 17, 1807.

THE court of assistants having received a letter from the Royal College of Physicians of London, addressed to this college, stating that his majesty had been graciously pleased, in compliance with an address from the honourable House of Commons, to direct his Royal College of Physicians of London to inquire into the state of vaccination in the united kingdom, to report their observations and opinion upon that practice, upon the evidence adduced in its support, and upon the causes which have hitherto retarded its general adoption; that the college were then engaged in the investiga-

* From the House of Commons printed Report of 8th July 1807.

tion of the several propositions thus referred to them, and requesting this college to co operate and communicate with them, in order that the report thereupon might be made as complete as possible:

And having, on the 21st day of November last, referred such letter to the consideration of the board of curators, with authority to take such steps respecting the contents thereof as they should judge proper, and report their proceedings thereon, from time to time, to the court: the board proceeded with all possible dispatch to the consideration of the subject.

The board being of opinion that it would be proper to address circular letters to the members of this college, with a view of collecting evidence, they submitted to the consideration of the court, holden on the 15th day of December last, the drafts of such letter as appeared to them best calculated to answer that end; and the same having been approved by the court, they caused copies thereof to be sent to all the members of the college in the united kingdom, whose residence could be ascertained, in the following form, viz.

"Sir,

"The Royal College of Surgeons being desirous to co-operate with the Royal College of Physicians of London, in obtaining information respecting vaccination, submit to you the following questions, to which the favour of your answer is requested.

"By order of the Court of Assistants,

Lincoln's-inn Fields, Dec. 15, 1806. "Okey Belfour, Secretary.

"1st, How many persons have you vaccinated?

"2d, Have any of your patients had the small-pox after vaccination? In the case of every such occurrence, at what period was the vaccine matter taken from the vesicle? How was it preserved? How long before it was inserted? What was the appearance of the inflammation? And what the interval between vaccination and the variolous eruption?

"3d, Have any bad effects occurred in your experience in consequence of vaccination? And if so, what were they?

"4th, Is the practice of vaccination increasing or decreasing

creasing in your neighbourhood? If decreasing, to what cause do you impute it?"

To such letters the board have received 426 answers; and the following are the results of their investigation:

The number of persons stated in such letters to have been vaccinated is 164,381.

The number of cases in which small-pox had followed vaccination is 56.

The board think it proper to remark under this head, that in the enumeration of cases in which small-pox has succeeded vaccination, they have included none but those in which the subject was vaccinated by the surgeon reporting the facts.

The bad consequences which have arisen from vaccination are, eruptions of the skin in 66 cases, and inflammation of the arm in 24 instances, of which three proved fatal.

Vaccination, in the greater number of counties from which reports have been received, appears to be increasing; it may be proper however to remark, that in the metropolis it is on the decrease.

The principal reasons assigned for the decrease are:

Imperfect vaccination;
Instances of small-pox after vaccination;
Supposed bad consequences;
Publications against the practice;
Popular prejudices.

And such report having been considered, it was moved, seconded, and

Resolved, That the report now read be adopted by this court as the answer of the court to the letter of the Royal College of Physicians of the 23d day of October last on the subject of vaccination.

Resolved, That a copy of these minutes and resolutions, signed by Mr. Governor Lucas (presiding at this court in the absence of the master), be transmitted by the secretary to the register of the Royal College of Physicians.

(Signed) WM. LUCAS.

L. *Re-*

L. *Report of the Royal College of Surgeons of Edinburgh on Vaccination**.

SIR, Edinburgh, March 3d, 1807.

THE practice of vaccine inoculation, both in private, and at the Vaccine Institution established here in 1801, is increasing so rapidly, that for two or three years past, the small pox has been reckoned rather a rare occurrence, even amongst the lower orders of the inhabitants of this city, unless in some particular quarters about twelve months ago; and, among the higher ranks of the inhabitants, the disease is unknown.

The members of the Royal College of Surgeons have much pleasure in reporting, that, as far as their experience goes, they have no doubt of the permanent security against the small pox which is produced by the constitutional affection of the cow pox; and that such has hitherto been their success in vaccination, as also to gain for it the confidence of the public, insomuch that they have not been required, for some years past, to inoculate any person with small pox who had not previously undergone the inoculation with the cow pox.

The members of the royal college have met with no occurrence in their practice of cow pox inoculation which could operate in their minds to its disadvantage, and they beg leave particularly to notice, that they have seen no instance of obstinate eruptions, or of new and dangerous diseases, which they could attribute to the introduction among mankind of this mild preventive of small pox. The Royal College of Surgeons know of no causes which have hitherto retarded the adoption of vaccine inoculation here; on the contrary, the practice has become general within this city: and from many thousand packets of vaccine matter having been sent by the members of the Royal College, and the Vaccine Institution here, to all parts of the country, the Royal College have reason to believe that the practice has been as generally adopted throughout this part of the United King-

* From the House of Commons printed Report of 8th July 1807.

dom

dom as could have been expected from the distance of some parts of the country from proper medical assistance, and other circumstances of that nature.

I have the honour to be, sir,
Your most obedient servant,
WM. FARQUHARSON, P. R. C. S. E.

LI. *Report of the Royal College of Surgeons in Ireland, on Vaccination*.*

SIR, Dublin, Feb. 4th, 1807.

I AM directed to transmit to you the inclosed report of a committee of the College of Surgeons in Ireland, to whom was referred a letter from the Royal College of Physicians in London, relative to the present state of vaccination in this part of the United Kingdom; and to state, that the College of Surgeons will be highly gratified by more frequent opportunities of corresponding with the English College of Physicians on any subject which may conduce to the advancement of science, and the welfare of the public.

I have the honour to be, sir,
Your most obedient humble servant,
JAMES HENTHORN, sec.

At a meeting of the Royal College of Surgeons in Ireland, holden at their Theatre, on Tuesday the 13th day of January 1807; Francis M'Evoy, esq. president.

Mr. Johnson reported from the committee, to whom was referred a letter from the College of Physicians, London, relative to the present state of vaccination in the United Kingdom, &c. &c. that they met, and came to the following resolutions:

That it appears to this committee, that inoculation with vaccine infection is now very generally adopted by the surgical practitioners in this part of the United Kingdom, as a preventive of small pox.

That it appears to this committee, that from the 25th day

* From the House of Commons printed Report of 8th July 1807.

of

of March 1800 to the 25th of November 1806, 11,504 persons have been inoculated with vaccine infection at the Dispensary for Infant Poor, and 2,831 at the Cow Pox Institution, making a total of 14,335, exclusive of the number inoculated at Hospitals and other places, where no registry is made and preserved.

That it is the opinion of this committee, that the cow pox has been found to be a mild disease, and rarely attended with danger, or any alarming symptom, and that the few cases of small pox which have occurred in this country, after supposed vaccination, have been satisfactorily proved to have arisen from accidental circumstances, and cannot be attributed to the want of efficacy in the genuine vaccine infection as a preventive of small pox.

That it is the opinion of this committee, that the causes which have hitherto retarded the more general adoption of vaccination in Ireland, have, in a great measure, proceeded from the prejudices of the lower classes of the people, and the interest of some irregular practitioners.

To which report the College agreed.

Extract from the minutes.

James Henthorn, sec.

LII. *Summary Considerations upon variegated Colours of Bodies when reduced into thin Pellicles; to which is added an Explanation of the Colours of tempered Steel, and of those of Peacocks' Feathers. Extracted from a Work on Colours; by* C. A. Prieur*.

Certain bodies of a very great tenuity, and the thickness of which varies progressively throughout their whole extent, exhibit, as is well known, a set of colours shaded in various and sometimes in very brilliant hues. It is not my intention to describe here these kinds of colours, and far less to dispute their peculiarities, so admirably described by Newton. I shall merely endeavour to draw some inferences upon the

* From *Annales de Chimie*, tom. lxi. p. 154. See Phil. Mag. vol. xxviii. pp. 162 and 210.

the origin of these colours, to establish their comparison with those owing to absorption, and finally I shall endeavour to ascribe to their true cause some phænomena, which to this hour have been differently explained.

I shall point out, in the first place, the principal effects upon which it is important for my purpose that the attention of my readers should be fixed.

When the light falls upon the very thin and slender bodies which yield these colours,

1st. At the spots where the colours arise upon the thin body, each complex bundle of rays, or, if you please, the white light, is divided into two portions, in a variable manner, and one of them is reflected while the other cannot come out of the substance except by transmission.

2d. This division varies according to a law depending upon the thickness of the body, its density, and the inclination of the luminous rays.

3d. Each ray acts in particular as if endowed with the singular property of having fits of easy reflection at periodical intervals, and fits of easy transmission at other intervals, alternate with the first: these various results are equally beyond all dispute.

But whence comes this disposition of the rays?—Newton considered it as inherent in the rays themselves, not only in that part of their transmission comprised between the two extreme surfaces of the body which they traverse, but also over the whole of the progress of these rays, from the moment of their first emission from a luminous body *. Here there is a kind of hidden cause, of which it seems very difficult to form a precise idea: some distinguished philosophers have therefore testified much repugnance in admitting it.

But Newton himself, at the end of his work, puts us in the right path when he asks, if it is not in virtue of a similar principle that the rays are reflected or refracted by bodies, and bent before they actually arrive at the bodies †.

We have much cause to regret that this great man has not

* Optics, book ii. part 3. prop. 12. † Ibid. book iii. quest. 4.

treated

treated the subject of inflexion with the same copiousness he has bestowed on his work upon coloured rings; or even that he did not turn his attention to the deviation of light in the adjacency of bodies, before examining its changes of direction from the action of surfaces: from these reflections he would certainly have drawn some very valuable consequences.

In fact, the greatest analogy exists between the phænomena of inflexion around a small body, and those of reflexion or transmission, by means of thin bodies; for the colours of the fringes in the one seem to follow the same law with that of the coloured rings in the others. And if this is not very perceptible with respect to the fringes adjacent to the shade of a body of small diameter received into a dark room, this similitude becomes more evident with respect to the fringes produced by the light which passes between two bodies very closely connected; it is still more so in the course of coloured images formed among the feathers of a quill, through which we look at the flame of a candle; and it is also very manifestly recognised in bars seen by the eye, when we place between us and the light, the texture of a piece of cloth, or a number of metallic wires placed close together, as in the experiments of M. Rittenhouse*.

I have myself found a method still more proper for producing this resemblance. For this purpose, I substitute in place of the piece of cloth, the black gauze called *crape*, in such a manner that if in a dark place we observe a light a little distant, by covering the eye with a black crape we see this light surrounded by a number of rings very apparent, and of very lively colours, shaded with the same hues with those of the coloured rings of thin pellicles.

If the flame of a candle is introduced into an aqueous vapour, a little abundant, where it cannot be seen except through this vapour, we see the flame surrounded with rings absolutely analogous to the preceding. I can imitate them also in an easy way, by tarnishing with my breath a plate of glass, which I immediately use for looking, either by reflexion or by transmission, at the image of a luminous body.

* Bib. Brit. Sciences et Arts, tom. ix.

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The coronæ we sometimes see surrounding the sun and the moon, are probably phænomena of the same kind.

In another place Newton speaks of undulated folds, like those of an eel, which he supposes to be produced in the rays, when they pass by the edges and sides of bodies *. I think I can explain the formation of these folds, and the necessity for their existence.

With this view, I shall call the attention of my readers to the very interesting results of the experiments of Newton and S. Gravesande, relative to inflexion: results so certain, that surely no philosopher can raise any doubts against them; but it is nevertheless satisfactory to be able to prove them ourselves, and to seize upon them with all their peculiarities, as I had the advantage of doing in some experiments upon this very subject made at the house of M. Tremery, in concert with Messrs. Berthollet, sen. and jun. These experiments I have detailed in a former part of my work.

From the action exercised upon the luminous rays by a point or by the edge of any given body, we are, in my opinion, warranted in considering each molecule or parcel of isolated matter as enveloped with a double sphere of activity relatively to the light; the one more interior, where the rays are attracted by the body, the other more exterior, where the rays are repelled. But it will happen, in several positions, that a ray coming to traverse the repulsive sphere, will therein describe a convex curve on the side of the body, that if it afterwards intersects the attractive sphere, the curve of deviation will then be concave with respect to the body, and that it will become a second time convex when the ray re-passes into the sphere of repulsion in order to continue its route. Here then is the commencement of the snaky movement, the folds of which may be multiplied by a string of molecules.

Will this cause be sufficient for operating the access of easy reflexions and transmissions of rays directed upon the surface of bodies?

The phænomena of colours, which we are here discussing,

* Optics, book iii. question 3.

seem-

seem to me to be explained very naturally by this method alone. Nevertheless I restrict myself to presenting it as a simple probability. In order to leave nothing further to be desired upon such a proposition, it will certainly be necessary to make a more profound examination of it, and particularly to try if we can apply any calculation to it, for the purpose of seeing whether, from the double virtue, attractive and repulsive, attributed to each molecule of any body, it would be possible, in a given case, to deduce the movement of the luminous rays reflected, or pushed, sometimes in one direction, and sometimes in another, conformably to the flexions or transmissions operated by the pellicles.

To conclude, it is more important to the object I have in view to remark, that the colours resulting from the fits of easy reflexions and transmissions, are produced equally, as was clearly seen by Mazeas, between the close connected surfaces of two bodies, without any substance whatever being between them *; for instance, between two lenses, or two glasses applied against each other and placed under the exhausted receiver of an air-pump.

On the other hand, these colours do not always require a very minute separation of surfaces, since Newton himself obtained coloured rings by the action of the two surfaces of a concave glass mirror, *three lines in thickness*, and found that these rings, with respect to *thick plates*, depended upon the thickness, following the same law which he had laid down with respect to *thin plates*; and this he again confirmed by the observation of the rings upon a mirror only *one line* thick †.

We see therefore, on referring to the various phænomena I have quoted, that the colours emitted by a pellicle or thin plate of glass, are as fugitive and independent of the colour proper to the substance, as those of a thick mass of glass; that these kinds of colours may not even depend upon the thickness of any substance, as when they arise in the interval of two close joined glasses, or in the fissures of certain minerals; that they have the greatest analogy with the coronæ produced through a fog, a smoke, or in the intervals

* Memoirs of the Berlin Academy, 1752. † Optics, book ii. part 4.

of

of a texture of threads, themselves impermeable to light, such as in black gauze or metallic wire cloth; and lastly, that on going back to the action of one point, or of a single material molecule upon the luminous fluid, we then find the most probable origin of the modifications of the direction of rays which twist or fold the molecules of bodies in the various examples cited, and which being influenced differently, each according to each species, escape by a different final direction: whence results a variety of colours upon these bodies, determined only by the number or distance of their molecules, and without any dependence upon their proper nature.

Let us now establish the parallel between these kinds of colours, and of those of the molecules of bodies submitted to the laws of absorption.

In the first place, with respect to the latter, the luminous bundles are not bifurcated as they are with respect to the former: the rays which do not re-appear in a given direction, are not thrown out in another direction; they remain absorbed in the substance, even when the mass is perfectly transparent.

In the second place, the colours resulting from absorption are sometimes owing to groups of rays very different from those which thin pellicles can furnish. For instance, the latter never furnish colours composed like those of bodies tinged violet by the oxide of manganese, nor like the blue of cobalt or of indigo.

Besides, there is no relation between the colouring depending upon the thickness of any body, in the two kinds of phænomena.

We may assure ourselves of this by a comparative examination of my table of the varieties of absorption *, and the

* This table, which is described and explained in a preceding part of the work, gives eight particular series of coloration, which serve as a type of all [illegible], the possibility of which is shown by nature in the phænomena of absorption. It also presents some very singular results upon the action of coloured bodies upon light.

We hope soon to communicate this part of the work at full length in a succeeding number of these Annals.—*Note by the Editors of the Annales de Chimie.*

 figure

figure so ingeniously traced by Newton in order to point out the colours of the rings *.

In the third place, the colours of the thinnest possible pellicles have a very great liveliness; those, on the contrary, of the most intense coloured solutions are imperceptible under an equally small thickness. It is for this reason that the colouring of small plates of mica of an excessive tenuity has no relation with the yellow colour of the mica in the mass from which these leaves have been detached. They are at that time in every respect similar to fragments of the most colourless glass blown into bubbles, nearly of an equal tenuity, and when mixed together we should have no sign by which to distinguish them.

Thus glass, mica, or any other substance, which under a great tenuity are decked in the most brilliant colours, pass, by increasing in thickness, to an absolutely colourless state, or to a colour totally independent of those they exhibit when in a thin state.

But, it will be said, in order to assimilate a coloured mass to an assemblage of parcels of a determinate thickness, it is also requisite that these parcels be kept at a convenient distance from each other.

In this case, I answer, you will have a certain colour reflected and another colour transmitted precisely complementary to the first. Now, this double colouring never takes place with respect to substances entirely diaphanous.

The examples of the infusion of nephritic wood, and of the precipitates of gold, are not applicable to this case, since here, as I have previously shown, the colours reflected are owing to molecules impermeable to light and disseminated in a transparent liquid, and because we may alter the nature of these molecules, or even have others of them in such a manner as to change the colour reflected, without there being any change on that account in the colour transmitted.

There has not hitherto been any case known, which permits us to consider a coloured body perfectly transparent, or even dull, as composed of parcels of a determinate thickness,

* Optics, book ii. part ii. plate 2. fig. 6.

and

and retained at distances necessary for producing a colouring dependent upon the thickness of these elementary parts.

In the fourth and last place, the colours of pellicles are in certain cases variable by the inclinations of light and of the eye, and sometimes also by the influence of the mediums with which we place them in contact. Nothing similar takes place with respect to the colours peculiar to molecules: fixed and permanent, in whatever direction we view them, they do not even change upon being introduced into a new limpid medium of a greater or less density.

These, in my mind, are enough of characteristic differences to authorise the opinion that the colours of bodies in masses have not the same origin with those of thin pellicles: a conclusion equally important with respect to its object in itself, as in respect to the disputes it still occasions among men of science*.

[To be continued.]

LIII. *An Examination of what* Jerome de Lalande *has published, in his History of Astronomy for* 1806, *concerning Dr.* Herschel *and his* 40-*feet Telescope.*

To Mr. Tilloch.

SIR,

Having long taken in your valuable philosophical miscellany, I lately met with the article called "The History of Astronomy, by Jerome de Lalande, for 1806," which has a place in your Magazine for July last. This is a new species of composition, which, it is well known, that author for several years past had been fond of: a kind of annual bulletin, consisting of a great farrago of topics and titles respecting the labours of his cotemporaries in astronomy and its kindred sciences, all treated of in a very cursory manner, with strictures on their merits not unfrequently superadded.

* See, among others, in the second edition of the Elements of Dyeing, by M. Berthollet, and in Haüy's Treatise upon Physics (second edition), the discussions and the contrary opinions of these celebrated authors upon this question.

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In thus flying, every new year, through such a variety of matter, and becoming as it were a Historian on the wing, there is some danger however of losing that distinct discernment of things below, which alone can bestow value on any such publication. This may happen by soaring too high, or by skimming too rapidly when nearer the surface; but most of all from that giddiness which sometimes overtakes very good people, when, either in reality or in imagination, they are lifted up far above the level of their fellow-mortals.

Such reflections, sir, spontaneously arose when, amongst the morsels of history above referred to, I perused the following paragraph concerning an excellent person, highly entitled to the regard of men of science, and who, by his genius and invincible perseverance, has so much enriched astronomy, and raised the fame of this country, by his manifold sublime discoveries in the heavens.

In page 129 of your Magazine for July last, the following passage makes up a link in Lalande's History of Astronomy for last year:

"The 40-feet telescope of Mr. Herschel has not yet furnished the extraordinary results we expected from it. I wrote to him that I was desirous of coming to England to visit this prodigious instrument, as soon as he wrote me that he had no objections: I have not yet received his answer. As Mr. Herschel is now 68 years of age, I am afraid he will not be able to satisfy himself, and that he will not find a successor capable of terminating completely so difficult an enterprise."

Now, sir, to continue for a moment longer the former allusion, I really should have expected, when so celebrated and so unique an object came in sight, that it would have arrested our historian in his airy career, and have lured him to hover a while in its zenith, that, by some competent examination, he might have represented it very differently in his bulletin. But in place of that, he brushes away after a short flourish, the evident tendency of which is to spread the belief, that Dr. Herschel has failed of success in constructing this noble instrument, so much exceeding all former example.

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The proof too of this defeat, as stated in the paragraph, turns, it should seem, on the circumstance of Dr. Herschel having made no reply to Mr. Lalande's letter. But this surely must be considered as a very summary, if not a cavalier, way of establishing the conclusion. Admitting that Mr. Lalande, when inviting himself to see the telescope, meant to do so with that respect which was due, yet it must be evident that he has been exceedingly unfortunate in the manner of his communication. By his own account of the matter, he prescribed the very singular condition which was to determine his journey to the telescope: namely, when Dr. Herschel should write him that he had no objections against his coming. Now, certainly there was something very ungracious in so putting it, and which warranted the inference of his prejudging Dr. Herschel's object, and of anticipating its failure, at the very time so much curiosity and interest was displayed about it in his letter.

Under these circumstances, it is most natural to suppose that Dr. Herschel's silence, if the letter in question ever reached him, proceeded from no want of confidence in his instrument, but more or less from disgust, in consequence of the application having been made in a way so peculiar and forbidding, as far as it could be construed.

But further: it comes in the next place to be particularly remarked, that this disparagement of Dr. Herschel's 40-feet telescope is very recent, notwithstanding many proofs of its utility and excellence have been published, many years ago, in the London Philosophical Transactions. What then must be thought of the article of the bulletin under review! As the most favourable construction ought surely to be made, it must therefore be supposed that the author had never seen or perused these volumes; though they have a wide circulation wherever science is cultivated. But such an omission necessarily implies that, in the present instance at least, the historian had had a very imperfect knowledge of his subject; otherwise, it is presumed that the paragraph would have had a very different complexion, by doing justice to Dr. Herschel's labours.

Indeed, I was so much surprised at this untoward paragraph,

graph, as being quite contrary to what I had long before read and believed of the 40-feet telescope in the garden at Slough, that I had again recourse to the Philosophical Transactions, in order to refresh my recollections about it. There we meet with accounts of Dr. Herschel's observations, with that telescope, on the planet Saturn; on its ring; on accurate measures of the ring; on its shadow on the body of Saturn; on one of the most curious nebulæ, &c. most evidently proving the paramount excellency of this telescope over his former ones, in regard to that kind of power and performance which he had steadfastly in contemplation in its original construction, and which his consummate experience and skill, in every thing relating to vision, pointed out to him as an acquisition well deserving of the most arduous pursuit.

But his success in this is rendered still more unquestionable by several passages, in the same volumes, where he gives an account of his discovering the sixth and seventh satellites of Saturn, solely by the assistance of that telescope, which could never have so availed him had Mr. Lalande's account of it been at all well founded.

Some of these passages shall now be quoted, that the reader, if he pleases, may confront them with the foregoing extract from Lalande's History.

From the Philosophical Transactions of the Royal Society of London for 1790, p. 10:—"In hopes of great success with my 40-feet speculum, I deferred the attack upon Saturn till that should be finished; and having taken an early opportunity of directing it to Saturn, the very first moment I saw the planet, which was the 28th of last August, I was presented with a view of six of its satellites, in such a situation, and so bright, as rendered it impossible to mistake them. The retrograde motion of Saturn amounted to nearly 4½ minutes per day, which made it very easy to ascertain whether the stars I took to be satellites really were so; and in about two hours and an half I had the pleasure of finding, that the planet had visibly carried them all away from their places."

Philosophical Transactions 1790, page 474:—"Sept. 17.

Forty-

Forty-feet reflector. I see six satellites at once; and being perfectly assured that the second is invisible, it becomes evident that Saturn has seven satellites. This new satellite is excessively small."

These two new satellites he observed many times afterwards, and determined their periods, &c. round Saturn. In another place we find the following passage:

Philosophical Transactions for 1800, page 76:—"Oct. 10, 1791. I saw the fourth satellite and the ring of Saturn, in the 40-feet speculum, without an eye-glass. The magnifying power on that occasion could not exceed 60 or 70; but the greater penetrating power made full amends for the lowness of the former, &c. Among other instances of the superior effects of penetration into space, I should mention the discovery of an additional sixth satellite of Saturn, on the 28th August 1789, and of a seventh on the 11th September, in the same year, which were first pointed out by this instrument."

Philosophical Transactions for 1800, page 77:—"Nov. 5, 1791. Forty-feet. I see the new sixth satellite much better with this instrument than with the 20-feet. The fifth also is much larger here than in the 20-feet; in which it was nearly the same size as a small fixed star, but here it is considerably larger than that star.

"Here the superior penetrating power of the 40-feet telescope showed itself on the 6th satellite of Saturn, which is a very faint object, &c. &c."

Philosophical Transactions for 1790, page 2:—"It may appear remarkable that these satellites should have remained so long unknown to us, when, for a century and an half past, the planet to which they belong has been the object of almost every astronomer's curiosity, on account of the singular phænomena of the ring. But it will be seen presently, from the situation and size of the satellites, that we could hardly expect to discover them till a telescope of the dimensions and aperture of my 40-feet reflector should be constructed; and I need not observe how much we, members of this society, must feel ourselves obliged to our royal patron, for his encouragement of the sciences, when we perceive that the

discovery of these satellites is entirely owing to the liberal support whereby our most benevolent King has enabled his humble astronomer to complete the arduous undertaking of constructing this instrument."

To conclude:—On a review of all Dr. Herschel's observations already made with this telescope, it appears, beyond contradiction, that he has fully succeeded in giving it the properties and superiority originally aimed at in its construction, how much soever Lalande has been led to disparage it.

Indeed, his two discoveries of Saturn's sixth and seventh satellites would of themselves be sufficient to render this telescope ever memorable, and surely ought to have prevented it from having been so much misrepresented.

ARCTURUS.

LIV. *On the Use of Zinc for covering Buildings.* By JAMES RANDALL, *Esq. Architect.*

To the Editor of the Philosophical Magazine.

SIR,

I SHALL feel myself obliged by your inserting the following account of an attempt which I have made to introduce zinc as a covering for buildings, and of the degree of success that has attendeded the undertaking. I am, sir,

Your obedient servant,

JAMES RANDALL.

Upper Charlton-street,
Fitzroy-square.

Zinc is one of the metals that has been hitherto thought unfit for any of the purposes to which the malleable metals are commonly applied. It is said to possess too much brittleness and want of ductility. Messrs. Hobson and Sylvester, of Sheffield, assert, "that at a temperature between 210° and 300° of Fahrenheit it is really a malleable metal, that it yields to the hammer, and may be wire-drawn if kept at this temperature during the operation; they say likewise, that after having been thus annealed and wrought, it continues soft, flexible, and extensible, and does not return to its

its previous brittleness, but may be bent and applied to many uses for which it has been hitherto thought unfit." These observations being now generally circulated, the manufacturers have used it; and with so much success, that what was before almost useless in the mechanical arts, is now wrought and sold in plates similar to copper. About two years since, I took an opportunity of trying whether or not it could be adopted for the purpose of covering buildings; for I conjectured, if it could, that it would be a most valuable acquisition to the public, particularly at the present time, when lead and copper are so exorbitantly dear as in some measure to supersede their use. I laid it on a temporary building (for experiment only) in bays, like the mode pursued with lead, and in a situation exposed to the most trying heat and inclemency of this climate, and no change is yet visible, except in colour, which is rather darker; nor does it appear at all oxidated; nor is there that appearance of unevenness in the surface, as is the case with thin lead and copper covering: its partial brittleness, however, makes it necessary to put it on in a different manner to what lead is, and in the shape of the roll only, which, instead of a plain round, should resemble that of an inverted cima-recta, which, by forming the internal angle into an easy hollow, will obviate the danger of cracking on hammering it down, which would almost invariably follow by adopting the other mode. I have been thus particular in describing this precaution, knowing the consequence of an error arising from the other plan. Zinc will be found extremely useful and œconomical for all kinds of pipes, gutters, &c.; and there can be no doubt of its being well adapted for the sheathing of ships, and, indeed, for almost all the purposes to which lead and copper are applied. Its hardness, according to Thomson, is $6\frac{1}{2}°$, when lead is only $5\frac{1}{2}°$; it melts at a temperature of 700° of Fahrenheit, while lead is fluid at 540°. It is well known in the arts, and has been the subject of much curious analysis by the chemists. It was called *zinc* by Paracelsus, who, there is reason to believe, was the first who succeeded in obtaining it pure; it has also been called *spelter*;

spelter; but the former name is retained among the learned: it was known to the Romans, who used it chiefly as an alloy, in which way it has been always found to possess very singular properties.

The expense of it as a covering is comparatively small, as, in the experiment above alluded to, after every care had been taken to lay it properly, the whole charge (including the alteration of the rolls) did not exceed one shilling and threepence per superficial foot, and it may be presumed that this was dearer than it would be when practice has given facility to the execution. From this it appears that it is almost as cheap as any covering, which promises durability, and not more than *one-third* the price of lead. The architect may also avail himself of its lightness to œconomize in the construction of his works.

LV. *Letter from Mr.* Parkinson, *of Hoxton-Square, relative to Mr.* Donovan's *Museum.*

To Mr. Tilloch.

SIR,

Permit me, through your excellent publication, to acknowledge my obligations to Mr. Donovan for the advantages I have derived in my inquiries respecting the mineralized remains of the animals of the former world, from the examination of the inestimable fossils contained in his matchless museum.

By the investigations which I had previously made, and from specimens in my own collection, I had ascertained that England alone yielded several species of *encrinities*, as I trust I shall show in the second volume of "Organic Remains of the former World," now in the press. But by an examination of the series of fossils in this department of the London Museum, as above mentioned, I have gained the knowledge that our own country can boast of yielding at least one additional curious species of this animal hitherto, I believe, unknown, and forming by the length of the arms

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an intermediate species between the lily and plumose encrinus. The specimen of which I speak is numbered 924 in the brief catalogue which is delivered at the museum.

From another specimen in the same collection, marked No. 950, I also acknowledge having derived very considerable information respecting the structure of that wonderful lost animal the tortoise encrinus.

Having no reason for concealing any of the motives which induce me to trouble you with this request, I do not hesitate to avow that one of these is a wish to call the attention of the curious, as well as scientific, to the most complete collection of British natural history which has ever yet been formed; a museum, not confined to any one particular branch, but comprehending alike the three great departments of nature, zoological, botanical, and mineral productions of the island, upon the grandest scale imaginable. It will not be too much to say that this museum, from the science evinced in its arrangement, independent of its importance as a collection of choice and valuable specimens, must, to those desirous of such knowledge, prove a most instructive school, and afford an inexhaustible fund of information to all those who think the natural history of their own country worth attending to.

I am, sir,
Your most obedient servant,
JAMES PARKINSON.

Hoxton-square,
Sept. 16, 1807.

LVI. *Essay upon the Art of the Foundry among the Antients: with some Remarks upon the celebrated Horses of Chio, now brought from Venice to Paris.* By M. SEITZ.

[Continued from p. 203.]

Art of Founding among the Romans.

THE Romans were as little skilled in the founding of metals as in all the other imitative arts. The Etruscans, who founded their statues under the kings, were employed in the

the same business under the republic. Thus Spurius Camillus, the conqueror of the Samnites, caused an Etruscan artist to cast a colossal statue of Apollo in bronze. It was the same statue which was afterwards placed in the library of Augustus. These haughty Romans, always at war with their neighbours and accustomed to conquer, thought it unworthy of their character to meddle with the arts; it was even forbidden the citizens to trade or to exercise a calling*. All the manual arts were therefore abandoned to foreigners and slaves.

This military spirit entirely predominated until after the second Punic war; having then become acquainted with the Greeks, they began to display some taste for the arts; at least, if we may judge of their taste from their despoiling the conquered nations of their ornaments, and decorating their own capital with them. In the public place of Tarentum there was a colossal statue of Jupiter made by Lysippus, and the second in magnitude after that of Rhodes. When Fabius Maximus Verrucosus retook this city from the Carthaginians, he wished to carry off this Colossus, but the difficulties attending the carriage prevented him †; he therefore contented himself with a Hercules, which was placed in the Capitol. Sixty years afterwards Marcellus took the city of Syracuse, and sent to Rome the masterpieces of art which decorated it. We find, from a discourse put into the mouth of Cato by Livy, how great an impression was made upon the Romans by these prodigies of the Grecian artists. "I see but too many," he says, "who exhaust the language of rapturous eulogy upon the masterpieces of Corinth and Athens, which our victories have procured us, while they regard with a disdainful smile the clay figures of the Roman gods placed at the entrances into our temples ‡." Lucius Scipio and Flaminius, the conquerors of Antioch and of Philippa; Paulus Emilius of Persia; Mummius of Corinth, and of several other cities of Achaia and of Boeotia; and lastly Scipio, the destroyer of Carthage, filled Rome with a prodigious quan-

* Dion. Halicar. book ix. † Strabo, book vi.

‡ Livy, book xxxiv. ch. 4.

tity of objects of the Grecian arts. The legacy which king Attalus, when dying, bequeathed to the Roman people, put them in possession of all Asia, and finished the entire corruption of their manners. Nevertheless, in spite of the introduction of luxury, the haughtiness of the Romans and their repugnance to labour were the same. They had seen monarchs chained to the triumphant chariots of the Roman generals; and if they formerly considered themselves a nation of heroes, they now imagined themselves superior to kings. The Greeks were their sculptors, their modellers, and their founders. The small number of Roman names found upon the antient works is sufficient to convince us of this*.

Under Augustus, while all the other arts flourished, that of the founders began to decline. He ordered four bronze elephants to be made, in order to decorate the *via sacra*. These elephants were not melted, but wrought with the hammer. We find a proof of this in Cassiodorus †. This minister of Theodatus, king of the Goths, 500 years after the reign of Augustus, wrote in the following manner to the præfect of the city of Rome:—"The bronze elephants are in a ruinous state; it would be a pity if these animals should exist a shorter time in bronze than they are said to do in a natural state. In order to restore this longevity to them, we must close the openings, and join the seams by means of iron cramps. We must also shore up their bellies outside, in order to preserve these works from ruin; the effect of which would be the more disagreeable, as their appearance was so imposing while they were entire."

Melted or cast elephants would not have been in such a state of decay in 500 years. Although this fact does not prove that the Romans at that æra were incapable of founding these elephants if they wished it, yet it is supported by

* Plin. lib. xxxvi.

† Lib. ii. ep. 38. Elephantes æneos vicina ruina titubare, his providentia vestra reddi faciat propriam longævitatem, uncis ferreis hiantia membra solidando, alvum quoque demissum pariete corroborent, ne illa magnitudo mirabilis solvatur turpiter in ruinam.

the

the testimony of Pliny *, who says that the art of founding large statues at one cast was lost in his time.

Among the artists celebrated in the days of Nero, Zenodorus, a Greek by birth, merits particular attention. He made a colossus of Mercury, in the city of the Auvergnates, which surpassed in size all the colossi then known. This work cost ten years labour, and about four millions of French money. After having proved his abilities by this memorable essay, he was called to Rome by Nero, where he made a colossal figure of that emperor 110 feet high, which was placed in the vestibule of his golden palace. Pliny says that people were wont to admire, in the workshop of Zenodorus, not only the model in clay regularly resembling the human body, but also the very small detached pieces destined to be put together, all of which, though in this unfinished state, gave a rough idea of the size and character of the performance †. We may presume, from these expressions of Pliny, that the above was not a statue cast at once, but composed of several pieces of hammered metal. Nero would no doubt have preferred having his image cast in bronze at one single jet, however great the expense a similar work might require; but the art of founding was lost. In the days of Pliny, who flourished under Vespasian, the Roman artists were still very far from having attained any thing like the perfection visible in the antient bronze works.

Soon after the time of Pliny, however, Domitian procured his statue to be founded by a Greek artist of the name of Celon, who, from the magnitude of his enterprise at least, deserves to be placed among those rare men of genius who knew how to revive an art which was thought to have been lost. Martial celebrated this statue in his epigrams, and Statius ‡ consecrated a whole eclogue to him. We learn from this last poem that it was an equestrian statue melted,

* Lib. xxxiv. c. 2.

† Mirabamur in officina, non solum ex argilla similitudinem insignem, verum ex parvis admodum surculis, quod primum operis instar fuit. Plin. lib. xxxiv. c. 7.

‡ ——————Celone peractum
Fluxit opus. Statius in Sylvis, lib. i.

placed

placed upon two grand pedestals; that the feet of the horse rested upon the figure of the Rhine, represented as a captive lying on the ground, and upon the figure of one of the Dacii in a similar dress and attitude: in short, this colossus exceeded in height the temples of the public square it occupied. The scaffolding and the machinery for transporting and raising this enormous mass were a spectacle, according to the same poet, as novel as it was curious, and all the young men in Rome were employed in raising it. Upon the death of the tyrant, indignation and hatred gave way to every other consideration, and this wonderful colossus was destroyed by order of the senate.

Under the emperors, all employments which did not lead to a speedy fortune were disregarded. "Labour, study the laws, exercise yourself in pleading causes," says a father to his son, in Juvenal*, "nothing else leads to preferment; or if you would rather choose to scale and demolish the castles of the Arabs, ask the emperor for a centurion's truncheon, you will perhaps obtain the command of a legion as the reward of your services, but this will be when you are sixty years old. If you prefer trade, become a skin-dealer, and let not the bad smell of the hides deter you; the gold you earn will place the perfumes of Arabia within your reach. Every where around you people will ask if you have got money, but none will inquire how you came by it." Such was the manner in which riches were spoken of. The poor, who had not the means of aspiring to fortune, nor the courage to attain it by dint of labour, attached themselves to an opulent man. They went every morning and paid their reverence to their patron, in order to obtain food from him for the rest of the day. The lower orders lived, for the most part, upon the distributions of corn made by the emperors, and by the liberalities of the ambitious, who, by this means, bought the suffrages of the common people. Let us add to this the number and the duration of their public festivals, the time which was wasted in the elections of their magistrates, and we may easily conceive that three-fourths of the people lived in absolute idle-

* Satire xiv.

ness,

ness, and that the Romans, under the emperors, had the luxuries of life too much at their command to be industrious. After this we need not wonder that the emperor Adrian was agreeably surprised when he saw that every person in Alexandria was busy*.

If the monuments in marble have sometimes escaped the fury of the barbarians, those in bronze have seldom outlived their cupidity. The two remarkable monuments which remain to us from the Romans, are the equestrian figure of Marcus Aurelius and the statue of Septimus Severus, which we see in the palace Barberini. M. Falconnet†, an excellent statuary, has repaired the defects of the former, which is founded in pieces‡. The statue of Severus is a fine piece of workmanship, and infinitely superior to the bas-reliefs we see upon the arch of Severus§. Whence does this difference arise? We know that this emperor was very parsimonious, in order that he might fill his treasury, and leave to his children the means of sustaining the commanding attitude he had given to the empire: it seems, therefore, that the sculptors of his arch were checked by this œconomy.

Nevertheless there were instances in which he displayed as much liberality as magnificence, particularly when he thought his glory interested. On one occasion he was anxious to see represented the subject of one of his dreams, to which he gave implicit faith. He had dreamt that he saw Pertinax adorned with all the attributes of the imperial dignity, and mounted upon a magnificently caparisoned horse, passing along the *via sacra*. When the horse came near where Severus stood, he threw his rider and halted before Severus, who mounted, and the animal carried him to the public square, amidst the acclamations of the people assembled. The subject of this dream was to be represented in the *via sacra* by a group of bronze figures worthy of ap-

* See the letter from Adrian to Severinus when consul, referred to by *Flavius Vopiscus in vita Saturnini*, c. viii. tom. 2. p. 719.

† Observations sur la Statue de Marc-Aurele, addressées à M. Diderot, par Etienne Falconnet.

‡ Memoires de Trevoux, Juillet 1703, p. 1208.

§ Letters of M. Winckeman, art. 7. vol. iii. of the History of the Arts.

pearing

pearing beside the beautiful monuments which already decorated that place. No expense seems to have been spared; Severus employed the best artists, paid them liberally, and they soon finished a colossal groupe in bronze, which represented Pertinax as thrown down, and Severus mounted upon the horse which had thrown off his predecessor*. It is not known if the colossal statue in the Barberini palace has any reference to this groupe; but it is certain that the beauty of the one must have had some influence upon that of the other, since contemporary works always resemble each other in their manner of execution.

Twenty-four years after the reign of Severus, the tyrant Maximian caused to be melted and converted into money a great part of the statues of the gods and heroes which decorated the city and its temples, without respecting either the antiquity or the beauty of the performance. The common people, who had willingly forgiven all the depredations he committed upon the property of the rich, were thus attacked in the most sensible point. When these fine monuments were destroyed, their grief was so great that several, guided by a blind zeal, were bold enough to resist, and preferred perishing before the statues of their gods rather than witness their destruction †.

The number of bronze statues was so great, that this diminution is still scarcely perceptible. Authors content themselves with informing us that they were innumerable, and that they might be compared to the living population of the country. The unknown author of the description of Rome ‡, who lived under Honorius and Valentinian, in these days could count twenty-three colossal and eighty gilt horses in Rome. He passes over in silence those which were common, and not gilt; but their number must have been very considerable, since the rich orators of Rome decorated the vestibules of their houses with bronze quadrigæ §.

* Herodian, lib. ii. § 34. † Id. lib. vii. § 8.

‡ Muratori, *Novus Thesaurus*, vol. i. at the beginning.

§ Juvenal, sat. vii. ver. 125.

Æmilio dabitur quantum petet——
———Hujus enim stat currus aheneus, alti
Quadrijuges in vestibulis, atque ipse feroci
Bellatore sedens curvatum hastile minatur
Eminùs, et statuâ meditatur prælia luscâ.

 I shall

I shall not enter into any further detail upon the bronze statues of Rome, since this subject has been treated by Winckelman, who has made particular inquiries as to the causes which contributed to their destruction.

[To be continued.]

LVII. *Letter from* Ezekiel Walker, *Esq.*

To Mr. Tilloch.

SIR,

The ninth number of the Retrospect contains some further observations on my paper in the xxivth volume of the Philosophical Magazine, p. 240, which might have been passed over without any reply, had not the writer used my silence as an argument to persuade his readers that I had drawn my facts from Berkeley's Theory of Vision.

This writer tells us, p. 236, that "Mr. Walker attempts, as he says, to demonstrate a certain property of the eye in two ways. In the first he makes a parade of algebraical substitutions, although there is no bringing of terms to an equation, no transposition, no extrication of known from unknown terms; yet, after all, he does not demonstrate the property of the plano-convex lens, but merely says in a note that it is well known to mathematicians.

"The second demonstration is vague and loose in the extreme; and where he reasons from the assumed case of a plano-convex lens to that of the lens of the eye, none of which are plano-convex, he evidently shifts his hypothesis before the conclusion is drawn.

"We have already said that Mr. Walker's pretended explanation is only an elucidation of that heretofore given by Berkeley, and we cited a passage from section 68 of that author's New Theory of Vision. Now, although Mr. Walker carefully avoids any allusion to what Dr. Berkeley has done, we desire our readers would compare Mr. Walker's additional facts in his last letter with the 70th and 71st sections of Berkeley, and they will at once perceive the source whence these facts were drawn."

In answer to these observations I beg leave to say, first, that

that the author ought to have known, before he began to write on optics, that the demonstration of the theorem, which is the foundation of my Theory of Vision, is to be seen in almost every book of optics that has been written in the English language since the days of Newton; and that I have not made a greater "parade of algebraical substitutions" than those writers have done. He ought also to have known, that it is the common practice of mathematicians to use a well known theorem without its demonstration.

Secondly, if what is called a demonstration contains any one step that "is vague and loose," it is no demonstration at all. Had this reviewer of philosophical papers found any step in my demonstration that is not strictly and rigidly true, he ought to have pointed it out, and advanced some better arguments against it than *his own bare word.*

Thirdly, "The aberration in any lens by the different refrangibility will be nearly the same as in a plano-convex of the same focus.

"For in a double convex lens of equal radii, the refraction at each surface will be but half so much as at the convex surface of the plano-convex, which has double the curvature *."

Whence it is evident that I did not shift my hypothesis before I drew the conclusion.

Lastly, As I never saw Berkeley's New Theory of Vision, I could not draw any facts from it; my theory is founded on the discoveries of a greater philosopher than even bishop Berkeley.

I am, sir,

Your most humble servant,

Ez. Walker.

Lynn,
Sept. 17, 1807.

* Emerson's Optics, p. 152.

LVIII. *Report of Surgical Cases in the Finsbury Dispensary, from the Beginning of Nov.* 1806 *to the End of Jan.* 1807, *with Observations on a Case of Hernia, attended with peculiar Symptoms, in which the Operation was performed with Success.* *By* JOHN TAUNTON.

In the last Surgical Report from this Institution (see Philosophical Magazine, vol. xxvi. no. 103.), there were 122 patients under cure; during the succeeding three months 275 persons were admitted:—

Cured - - - - - - - - -	366
Relieved - - - - - - - -	26
Under cure - - - - - - -	5
	397

J. L. ætat. 46, by trade an ivory-turner, naturally of a delicate constitution, has been a long time subject to an asthmatic cough, and has also laboured under inguinal hernia in both groins for many years. On visiting him on Tuesday afternoon, Nov. 4th, I learnt the following particulars: on Friday Oct. 31, the hernia on the right side came down and could not be returned; the pain was considerable and continued, though he had an evacuation by stool on Saturday; the pain was much increased on Sunday, attended with hiccup, sickness, and vomiting; the symptoms being more urgent on Monday, he was visited by a physician, who ordered a purging mixture, with tincture of opium, to be taken, and fomentations to be applied to the abdomen; the mixture was rejected by the stomach as soon as taken, and the fomentation did not afford the least relief; an enema was injected and retained in the evening, but without producing any effect.

The symptoms were now become *very urgent*, a distressing *hiccup*, and almost constant vomiting of *feculent matter*; the stomach rejects every thing as soon as taken; pulse 110, weak and thready, or rather vibrating; the tongue covered with a brown fur; a dejected countenance, and the pain

pain at the navel and region of the stomach extremely distressing; the hernia was small, scarcely projecting beyond the external abdominal ring, and appeared to recede on pressure completely within the ring.

An infusion of nicotiana (ʒi. to a pint of water) was injected and retained without producing the least apparent effect; two grains of calomel and one grain of opium were given in form of pill every hour, but these were rejected as soon as swallowed.

At six o'clock in the afternoon the tobacco injection was repeated and retained, but apparently produced no effect; the operation being proposed, was acceded to, and performed at 10 o'clock at night.

The tendinous fibres of the external oblique muscle were separated longitudinally from the external abdominal ring towards the superior part of the ilium, when the hernial sac appeared of a dark blue colour and very tense, and contained some serous fluid, and one fold of the intestinum ilium, which was of a very dark colour, nearly approaching to gangrene: the stricture on the intestine being at the internal ring, rendered it of more difficult access, but it was dilated obliquely upwards and outwards; that is, in the direction of a line drawn from the symphysis pubes to the superior part of the ilium: at that part there were strong adhesions between the intestine and mouth of the sac; which being separated, the intestine was returned: but the patient did not experience that change of sensation which frequently occurs on the bowel being returned; the edges of the wound were brought into contact by two sutures, supported by straps of adhesive plaster; a gentle laxative medicine was given occasionally during the night, but could not be retained on the stomach, as the sickness and hiccup continued with unabated violence.

On the 5th, at 7 o'clock in the morning, the countenance was much improved, the pulse 92, the pain much diminished; but the hiccup, sickness, and vomiting, continued the same as before the operation; an enema composed of magn. vit. ℥iſs. ol. geno ℥i. gruel ℔iſs. m. was in-

jected

jected and retained; some pills of extract colocynth with calomel were given, but immediately rejected by the stomach.

At 12 o'clock the enema was repeated and retained, but produced no effect. At 10 o'clock at night the countenance was good, pulse 84, but there had not been any evacuation by stool; the hiccup, sickness, and vomiting continued; an embrocation of soap liniment and æther was applied to the umbilical region, and a table-spoonful of the following mixture was ordered to be given frequently during the night: ℞. æth. vit. ʒii. tinct. opii ʒi. aq. menth. ℥iv. m.

On the 6th, at eight o'clock in the morning, the pulse was 84, a soft skin, rather inclining to perspiration; the hiccup and sickness had nearly subsided, he had two copious evacuations by stool during the night, and had also slept for several hours.

At three o'clock in the afternoon the *vomiting* of *feculent matter* had returned, as violent as it had ever been before the operation; pulse 96, small and thready, the countenance more irritable, but there did not appear to be any tension of the abdominal viscera.

At eight o'clock in the evening the symptoms were the same, the medicine and embrocation were continued.

On the 7th there was no mitigation of symptoms; the treatment was the same.

On the 8th, the sickness and vomiting were somewhat abated, some sleep had been obtained, but no evacuation by stool; the following pills were taken during the day: ℞. calom. gr. viii. jalapi gr. xvi. m. f. pil. iv.

On the 9th, in the morning, the pulse was small and thready, the sickness was much abated, but the night had been restless, and no evacuation by stool had been obtained; the pills, ordered yesterday, were repeated: in the evening, copious evacuations by stool took place, at first hard, and afterwards fluid, and attended with griping pain; an opiate pill was given at bed-time.

On the 10th, the hiccup and sickness had subsided, the appetite began to return; the wound was dressed, which had nearly

nearly healed; from this time the cure went on gradually, and was completed in about a week.

In this hernia, the external swelling was but small, the sac being situated between the two abdominal rings; it receded easily under the edge of the transversalis muscle by external pressure, a *circumstance not sufficiently explained in surgical authors, nor known by surgeons in general.* A fatal mistake of this kind came within my knowledge but a short time since, in a patient who would willingly have submitted to the operation, but a surgeon persisted in his capability of reducing the hernia, because he found it to recede by external pressure: the patient died in a few days: dissection proved the fallacy of his judgment.

JOHN TAUNTON,
Surgeon to the City and Finsbury Dispensaries, Lecturer on Anatomy, Surgery, Physiology, &c.

Greville-street, Hatton-garden,
Sept. 21, 1807.

Subscriptions and donations for the City Truss Society for affording relief to the ruptured poor, by supplying them with trusses free of expense, are received by James Amos, esq. Devonshire-square, Bishopsgate; Mr. Alexander Maxwell, 331, Strand; Mr. Elliott, City Dispensary; Mr. Bartlett, Finsbury Dispensary; and by Mr. Taunton, where plans may also be had.

LIX. *Additional Memoir upon living and fossil Elephants.* *By* M. CUVIER.

[Continued from p. 264.]

WHEN all the parts of the body of the tooth are made and consolidated, and when it comes to emerge from its alveolus, it undergoes changes of a new description.

As the elephant is herbivorous, its teeth are worn down by mastication, like those of all the animals who live upon similar food. We even know, that it is necessary that their teeth should be worn before their surfaces are in a fit state

for chewing vegetable substances. This general fact, still more clearly brought to light by the labours of M. Tenon, proved to him of itself, and independently of all those which we are about to develop, that teeth are not organised in the same manner with bones. We all know to what accidents these last are exposed, when they are cut or merely laid bare.

The summits of the small notches of the laminæ will be first worn:—when once worn down to the interior substance, each of these summits will present a circular or oval disk of that substance, surrounded by a circle of enamel and a circle of cortical; and there will be a range of these small circles by each lamina.

If the detrition penetrates to the bottom of the hollows which produce the notches, all these small circles will be united into one single stripe of osseous substance, surrounded by a double line of enamel, and the cortical substance will make the whole tour of the tablet of the tooth, and will occupy all the intervals of the stripes. Each stripe will be the section of one of the transversal laminæ which compose the tooth.

And if the detrition could go the length of the place where the laminæ all join into a single corona, the entire tooth will no longer present any thing else than a very large disk of osseous substance, surrounded on every side by a small edge of enamel and another of cortical.

But the detrition can never go so far, because it never takes effect at the same time upon the whole corona, as the consolidation has not taken place there, and for the following reason:

The tooth, in consequence of its rhomboidal form, and from its very oblique position, presents its anterior part much sooner in mastication than it does its posterior part. The plane or tablet produced by mastication therefore forms, with the common surface of the summits of all the laminæ, an angle open behind; and from this it happens, that, when the front laminæ are cut deeply, and form entire stripes, the intermediate laminæ do not as yet present any thing else than transverse rows of circles or ovals, and those behind are

are completely untouched, and present the summits of their notches in the shape of nipples.

The anterior laminæ are also completely destroyed before the posterior ones are strongly cut in front; and from this there follows another phænomenon, also peculiar to elephants; their teeth diminish in length at the same time that they diminish in height.

While the exterior part of the tooth is wearing down and diminishing, the portion of the root which corresponds to it is worn in another manner, which is more difficult to conceive. On examining what remains of it, we find it as it were pared down; it presents at its surface small irregular cavities, as if it had been dissolved by an acid thrown upon it drop by drop. It is a kind of caries similar to that undergone by the human teeth when stripped of their enamel. We shall investigate the cause of this presently. It always happens that the tooth is by this means successively deprived, in the various portions of its length, of segments or trenches which occupy the whole height of it.

Thence also results another singular effect: the anterior part of the jaw being always to be filled up, the tooth moves from behind to the front horizontally, while it moves vertically from top to bottom, or *vice versa*, according as it belongs to the upper or lower jaw.

This is the reason why every tooth, at the time of its falling, is very small, however large it may have been previously.

This movement of the tooth makes room for that which is formed in the back jaw, and which must succeed it; this second tooth assists by its development in pushing the first forward; and we may say that the large teeth of the elephant come behind its young teeth, in place of coming above or under, as in other animals.

Patrick Blair*, who had seen separate transverse laminæ in the back jaws of the elephant, and who had named them very justly *rudiments of teeth*, was unwilling to believe that these laminæ afterwards proceeded to form a tooth which

* Phil. Trans. vol. xxvii. no. 326. p. 116.

would

would replace the one behind which he found these laminæ. He was therefore reduced to the necessity of searching for various imaginary uses of them.

Disputes were maintained as to the number of the teeth of the elephant: the Royal Society of London perceived, in 1715, that it varies from one to two on each side, and that the place of the division varies also; that is, the first tooth is longer or shorter, in proportion to the second, according to the individuals *. Pallas was the first who taught the mode of their succession, which explains all these irregularities, by showing that at first they have only a single tooth on each side; that the second, in developing itself, pushes the former in such a manner that during a certain time there are two of them; afterwards the fall of the first leaves one only †.

I have announced that this succession, and consequently this alternate change in number, was repeated more than once, because I had also found separate germs in an elephant which had already two teeth in their places ‡. This last point had been already established, but with respect to the upper jaws only, by Daubenton §; in short, this great naturalist had also presented to a certain degree the necessity of this succession from back to front, which Pallas has more clearly developed.

Mr. Corse || has informed us that this succession is repeated so often as eight times in the Indian elephant; that there are consequently thirty-two teeth which successively occupy the different parts of its jaws.

The first appear eight or ten days after birth; they are well formed in six weeks, and completely cut in three months. The second are well cut at two years of age. The third appear at this period, and make the second fall out at six years; these are, in their turns, pushed out by the fourth at nine years. The subsequent periods are not so well known.

For my part, I never found more or less than three teeth at once in the two elephants I dissected, and in five dry skeleton

* Phil. Trans. vol. xxix. no. 349. p. 370.

† Nov. Com. Petrop. xiii.

‡ Memoires de l'Institut, Sciences Math. tom. ii.

§ Hist. Nat. tom. xi. in 4to.

|| Phil. Trans. 1799.

leton heads which I examined, viz. a small grinder more or less ready to fall; a tooth in its place, and full grown; and a germ more or less large, more or less consolidated, and occupying the whole bottom of the back jaw.

We may easily judge, from the depth of the detrition, whether a tooth which has been found isolated, was situated before or behind in the jaw; those which were situated in front never have any of their laminæ entire.

The number of the laminæ which compose each tooth go on augmenting in such a manner, that each tooth has more laminæ than that which immediately preceded it.

Mr. Corse, who first made this remark, gives these numbers from his own observations *: the first have four laminæ only; the second, eight or nine; the third, twelve or thirteen; and so on to the seventh or eighth, which have 22 or 23 laminæ. Mr. Corse never saw any teeth which had more.

We have reason to believe that these numbers are not very constant; for we have a lower jaw, the first tooth of which has 14 laminæ, and the subsequent one has fourteen germs of laminæ. M. Camper has one completely similar (Descrip. Anat. d'un Eleph. Plate XIX. fig. 2.); but in the upper jaw, which corresponds to ours, there are in the full grown tooth thirteen laminæ, and in the germ of the subsequent one there are eighteen.

Independently of the number, there are deficiences with respect to the thickness of the laminæ; they are thinner in the first teeth than in the last: and as the jaws are shorter when they bear the first teeth, it happens that the number of the laminæ in activity is nearly the same at all times; *i. e.* from 10 to 12.

When the elephant is full grown the space occupied by the laminæ in activity, is larger, it is true; but these laminæ are themselves larger, and always fill the space whatever it may be.

As it requires nearly the same time to wear down the same number of laminæ, the last teeth, which have much more

* Phil. Trans. ut supra.

of

of them, last much longer than the first. The substitutions therefore take place at intervals longer and longer as the elephant advances in age.

The teeth of elephants, like those of all other animals, only push out their roots when the body is perfect; the roots are formed in layers like the rest of the tooth: it could not well be otherwise. But why this division in another sense, when the junction of the caps of all the gelatinous eminences seems to produce nothing else than a single body?

In order to answer this question, which is of a general interest to all kinds of teeth, we must add a circumstance to the description I have given of the genus: I have reserved this point until the present occasion, that I might not too much confuse the ideas of my readers.

The base of this gelatinous body, whose productions, which I have called *walls*, serve as nuclei to the laminæ of the tooth, does not adhere at all its points to the bottom of the capsule. There are, from space to space, interruptions of the continuity, and consequently the adherent parts of this base may be considered as very short pedicles. When the laminæ of osseous substance cover all the productions or walls, and all the body of the nucleus of the tooth, it is continued always upon and between the pedicles: the particles of this lamina which proceed between the pedicles form the under part of the body of the tooth; the particles which envelop the pedicles, and which are consequently more or less tubulous, form the first commencements of the roots.

These roots, and the pedicles which serve them as a nucleus, are afterwards lengthened, for two reasons: at first, the progress of the laminæ of osseous substance, which, by always shooting, force the tooth to rise and leave the alveolus; afterwards, the thickening of the body of the tooth by the formation of the successive layers, which, by filling the interior vacuum, hardly leaves any more room for the gelatinous nucleus, and pushes it towards the interior of the tubes of the roots.

There is no enamel nor cortical produced upon the roots, because

because the internal lamina of the capsule, which has alone the power to secrete these two substances, does not extend so far.

I am of opinion that it is partly to this absence of enamel that the corrosion is owing, which begins upon the roots as soon as the portion of the corona corresponding to them is worn down their length.

At this period the root has assumed all the development of which it is capable; the pulpy nucleus is entirely repelled by the layers with which it has itself filled the cavity it occupied. This force of increasing of the root ceases therefore to counterbalance the increase of the osseous partitions of the alveolus, and the latter continually push the root outwards. It begins to rot as soon as, when showing itself out of the gum, it is exposed to the septic action of the air, the heat and humidity of the mouth.

What, in my opinion, adds some probability to this idea is, that the corrosion begins rather at the junction of the root and the corona than at the point of the root. I have several proofs of this in my specimens. We may also judge of it from the small tooth represented by Mr. Corse. (Phil. Trans. 1799, Pl. VI. fig. 3.) Perhaps also the mechanical compression which the root experiences from the alveolus contributes to its destruction, as we attribute the destruction of the roots of the young teeth to the compression they receive by the shrinking of their alveolus, occasioned by the development of the teeth which must succeed them.

To conclude, one part of their molecules must be organically absorbed; but this would not be the only phænomenon in which a body, become foreign, is sucked up by the lymphatics, and disappears. The fact is known with respect to liquids. As for solids, I think we have examples of it in some remote instances. We may consult on this head the dissertation by Alexander Macdonald.

The teeth of the two jaws of the elephant are easily distinguished by their form. Those of the upper jaw have their laminæ disposed in such a manner, that all their summits are in a convex surface. The tablet produced by their detrition

detrition is also convex. The contrary is the case, in these two respects, with the lower jaw.

A still more striking character may be inferred from the direction of the laminæ, with respect to the corona on the triturating part.

Those from below are inclined backwards; that is to say, the acute angle they form with the plane of trituration is directed forward, at least in their radical part; for the summits of the anterior laminæ are bent a little backward.

Those from above, on the contrary, are inclined forward, or the acute angle they form with the plane of trituration is directed backward.

It is always easy to distinguish the back of the tooth from the front: the trituration cutting much more forward than backward, it is the end the most deeply worn down of the corona, which is always the anterior.

It must be remarked, however, that the inclination of the laminæ upon the corona diminishes at the two jaws in proportion as the detrition increases. The posterior laminæ, which are not so slowly worn down, are now worn a little faster, because, their development towards the root continuing when that of the anterior laminæ has ceased, they are pushed outwards with more force: whence it happens that the tablet of detrition becomes more and more perpendicular to the direction of the laminæ.

We also distinguish the teeth belonging to each side, because they are convex to their internal face, and a little concave to the external one.

[To be continued.]

LX. *Proceedings of Learned Societies.*

FRENCH NATIONAL INSTITUTE.

[Continued from p. 284.]

M. DE BEAUVOIS entertains similar ideas upon the impregnation of mushrooms.

Various parts of these plants, like the laminæ of the agarics,

ries, the points of the hydnæ, &c., are covered at certain periods with a multitude of small grains, or powder; other genera, like the lycoperdons, have their interior filled with this powder, and squirt it out at the period of ripeness. These grains are regarded as the seeds, or as their capsules; at least, this is the opinion of all botanists who think that mushrooms have seeds. M. de Beauvois maintains, on the contrary, that this is the *pollen*, and says that the seeds are in the interior of the laminæ or of the points, or rather in some other part of the texture, and that they have hitherto escaped the eyes of his predecessors because they are almost invisible. It is therefore at the moment of the explosion, and consequently when they are already developed, that he thinks the grains of the lycoperdons, as well as those of the mosses, are fecundated.

Such is the system according to which M. de Beauvois thinks himself authorised to erase the name of *Cryptogamia*, or of *hidden fructification*, given by Linnæus, and also preserved by Hedwig, to these different families, and to substitute the name of *Ætheogamia*, or *plants of an unusual or extraordinary fructification.*

He has published one part of his *Prodromus of Ætheogamy*, a pamphlet in which he announces the distribution he establishes among the mosses; he has separated in the formation of the genera what Hedwig takes for the organs of the male sex; a precaution so far proper, as the functions of these parts are not yet beyond controversy; and he uses the same caution, although in opposition to himself, by not giving any account of this columella, which he takes for the pistil. Nevertheless, it is according to the organs of the sexes that he separates in this same prodromus the *lycopodæ* of the common mosses; but this is because he thinks there remains no doubt with respect to the former, at least in some genera.

In a second part, still in manuscript, but read to the class, M. de Beauvois presents his distribution of mushrooms and of the *algæ*. In the former he has made some changes in the distribution of *Persoon*, and he reduces the number

number of genera from 71 to 60, which he distributes in six orders.

In a more recent memoir he asserts that he saw upon some young plants grains which appeared to him to be similar to the seeds of the parasite mushrooms, which are accustomed to develop themselves in the substance of these plants and under their epidermis: from this he draws a conclusion contrary to a memoir of M. de Candolle, of which we shall speak shortly, that these grains traverse the epidermis in order to lodge themselves below it. He further dwells on certain living mushrooms, which grow by layers from top to bottom, contrary to other vegetables: this observation was made long ago by Marsili and Bulliard; but M. de Beauvois adds to it the idea that each layer may be considered as a particular individual, or as a new mushroom proceeding from the grains of the anterior layer.

Lastly, M. de Beauvois has shown that there are considerable differences between the flowers of the *raphia* of Oware, and those of the *sagoutier* of the Moluccas, so that we should no longer leave them in the genus of palm-trees, as hitherto done; and he has communicated the description of two *lobeliæ*.

Among the less fortunate candidates, only two of them, Messrs. de Candolle and Du Petit-Thouars, have presented new memoirs on this occasion.

M. de Candolle, although still very young, has enriched with discoveries as numerous as they were interesting, vegetable physics, botany properly so called, and the materia medica.

To the first of these sciences belong the observations he made upon the action of artificial light, which acting at first but insensibly, proceeds at last to change completely the habits of vegetables: his observations upon the cortical pores; upon the production of oxygen gas by the green lichens, which had been denied, but of which he showed the reality; lastly, upon the vegetation of the misletoe, which attracts very fast the sap of the apple-tree, while it cannot suck up water into which it is suddenly plunged; a fact which

which modifies the ideas entertained upon the causes of the ascension of the sap.

To descriptive botany belong his history of the oily plants, of the *liliaceæ*, of the *astragal*, the edition of his French Flora, which he published under the eye of our colleague M. de la Marck, and various memoirs on particular subjects; works which have enriched the catalogue of vegetables with 37 genera, and more than 300 species formerly unknown.

Lastly, in the materia medica he was the first to distinguish the various vegetables confounded under the name of *ipecacuanha*, and those which are jumbled together under the name of *Corsican moss*; and in a treatise upon the *agreement of the virtues of plants with their natural families*, he has developed, according to new views, the rules to be followed in these sorts of inquiries; rules, the neglect of which has led into serious errors those who were occupied before him with this subject, one of the most important of practical botany.

To all these labours M. de Candolle has added three memoirs, which he presented to the class in the course of the last half year.

The first is on the subject of the *parasite mushrooms*, which are developed under the epidermis of vegetables, and which cause fatal diseases to several useful plants: the *rust* or *char*, which destroys corn, and the *caries* or rot, which poisons wheat, are probably to be ascribed to this cause. It has been hitherto supposed that these mushrooms were introduced by the pores of the epidermis; but as coloured liquors cannot pass through these pores without great difficulty, and as a simple application does not inoculate these diseases upon the plant, M. de Candolle thinks that their germs are introduced by the roots with the nutritive juices of vegetables, and circulate in the interior of the vessels, until they arrive at the places proper for their development; he compares them in this respect to intestinal worms, which cannot subsist except in the interior of the body of other animals: from this theory, and from the observation that each species of parasite mushroom can only propagate itself

in plants of the same family, he deduces rules from which agriculture may derive great advantage in stopping this kind of contagion.

Before M. de Candolle's time, we were acquainted with 84 of these mushrooms: his observations have increased this number to 100.

In a memoir upon marine *algæ* he shows that these plants have not any real roots; that in their organization there is no trace whatever of vessels; that they absorb humidity throughout their whole surface; that the more green they are, the more oxygen gas they liberate in the light; he announces that the small grains, hitherto regarded as their seeds, are only the capsules of them, and contain grains much smaller, coated with a viscous matter, which fixes them where they are to germinate.

Lastly, M. de Candolle has presented a memoir, in botany properly so called, upon the family of the *rubiaceæ*, which he divides into four orders, and to which he adds four new genera.

M. de Petit-Thouars has resided a long time in the isles of France and Bourbon, and has made a voyage to Madagascar. He has begun to publish the Flora of the latter, which is very rich in singular plants; in particular, he has made some valuable observations upon the *orchideæ*, plants which must be examined while in life, and which refuse all cultivation. He is ready to publish a great number of new species of these plants. The ferns have also been a peculiar object of his inquiries. The island of Madagascar alone supplied him with 89 new genera, the characters of which he is about to print, and which he sent into France ten years ago. His observations upon the germination of the *cycas* led him to discover that this singular tree, which some call a *fern*, and others a palm-tree, ought to constitute a distinct family.

The *dracæna* or *candle wood* has made him acquainted with particular facts, extremely curious, which led him to a general and new system upon the development of trees. We shall endeavour to give an idea of it.

We know that the trunk of ordinary trees increases from

4 the

the layers of wood, which are every year manifested under the bark, and that it is lengthened and ramified by shoots, which are merely the development of buds. Each of these new shoots has only a single layer of wood, which is in communication with the last of those formed upon the trunk; and the medullary thread, which occupies the axis of these shoots, comes from the marrow which reigns in the middle of the tree. Naturalists generally suppose that these successive ligneous layers grow every year under the internal face of the bark.

Palm-trees, and the other monocotyledonous trees, grow quite differently: the new fibres are developed in the axis and not in the circumference of the trunk; they traverse the whole length of this axis in order to expend themselves at the summit of the tree in leaves and flowers. This is the reason why the trunk of the palm-tree increases so imperceptibly, particularly below, and in general produces no branches.

M. Desfontaines, our colleague, has shown that this method of growing is common to nearly all the monocotyledonous plants, and distinguishes them in general from the dicotyledonous.

But, M. du Petit-Thouars having remarked that the *dracæna* (trees really monocotyledonous) ramified, as it were, like the ordinary trees, and wishing to account for this phænomenon, he ascertained by dissection that the axis of a branch did not communicate with that of the tree, but that the fibres of this branch, when arrived at the place of its junction with the trunk, blow out upon the latter, diverging like radii; the lower fibres descend directly; the upper ones, after having mounted a little, become crooked, and descend also. These trees therefore grow by concentrical layers, and, in fact, they become thicker the more they ramify. Such are the facts, and the following is the system:

M. du Petit-Thouars, applying these observations to all the trees with concentrical layers, concluded that the new layers are not produced by the bark, but the buds; that their fibres are descending prolongations of these buds, as the shoots are ascending prolongations. He thinks that the

 juice

juice contained in the marrow furnishes to the buds their first nourishment, as the cotyledons furnish it to the young plant: he is forced to add that these fibres are developed from the buds, which give them birth, to the roots, with a rapidity which he compares to that of light or electricity, because the ligneous layer is formed upon the whole extent of the tree in the space of a few days. The necessity of admitting so rapid a development is already, as has been seen, a strong difficulty against this opinion. There is another objection which seems still more forcible: where we engraft one kind of tree upon another, a pear-tree, for instance, upon an apple-tree, each species forms its own wood in the parts which come from itself; the graft has apple-tree wood only, and every thing above the insertion is pear-tree wood only. We may distinctly observe the place where the two woods are separated; and, as great care has been taken to strip the graft of its buds, it must necessarily follow, that its wood was furnished by the bark alone; for how, the partisans of the antient doctrine ask, can pear-tree buds produce apple-tree wood? It is, answers M. du Petit-Thouars, because the fibres which descend from these buds cannot be nourished in their passage along the trunk of the apple-tree, except from the juices furnished to them by the latter.

[To be continued.]

ROYAL SOCIETY OF SCIENCES AT GÖTTINGEN.

This learned body has offered a prize of fifty ducats for the best memoir on the following subject:—" The difference of colour remarked between the arterial and venous blood, has made several men of science believe that there exists a similar difference in the blood of the fœtus in an inverse ratio: but, as experience has not confirmed this opinion in any way with respect to new-born infants, the society desires that, by inquiries and direct experiments upon children born of healthy mothers, either by the prompt tying up of the umbilical cord at its two extremities at the moment of birth, or in any other manner, we should determine if there really exists an inverse difference in the co-

lour

lour of the blood; in what it consists; what are the constituent principles of the blood of the child, an abstraction being made of the acid parts, which should be mixed with it by its contact with the atmosphere."

The memoirs on the above subject must be transmitted to the society by the beginning of September 1808.

LXI. *Intelligence and Miscellaneous Articles.*

METEORIC STONE IN RUSSIA.

On the 13th of March last, in the afternoon, the inhabitants of the canton of Juchnow, in the government of Smolensk, were alarmed by an uncommon loud clap of thunder. At the moment of this explosion two peasants belonging to the village of Peremeschajew, in the canton of Werreja, being out in the fields, perceived at the distance of 40 paces, a black stone of considerable magnitude falling to the earth, which it penetrated to a considerable depth beneath the snow. It was dug up, and found to be of an oblong square figure, of a black colour, resembling cast iron. Its surface was very smooth, shaped like a coffin on one side, and it weighed about 160 pounds.

MISCELLANEOUS.

A traveller who lately arrived at Baltimore, in America, has brought from the banks of the Missouri an enormous tooth of a mammoth. He relates, that being occupied, along with others, in searching for mines in the neighbourhood of the river, they found the extent of a quarter of a mile square, filled, to the depth of six feet, with bones of an enormous size. The above traveller offers to produce, upon payment of a certain sum, a complete skeleton of a mammoth, which is 54 feet long and 22 in height. The middle toe of the fore foot of this skeleton is 7 feet 5 inches long. Each jaw has eight enormous grinders. The tooth brought by the traveller was presented by him to the Baltimore museum. What he has related as to the enormous quantity of

 bones

bones he saw may be exaggerated, but it would be interesting to make researches in the spot he has visited.

The Instructions for Vaccination, drawn up by the Vaccine Society of Copenhagen, have been translated into the Icelandic language by M. Thorarsen. It is expected that by this means the ravages of the small-pox will be entirely stopt. The work has been printed, and the copies distributed, gratis, throughout Iceland, accompanied with engravings.

It has been mentioned in some of the foreign journals, that M. Klaproth, the orientalist, had set out for Pekin with the Greek missionaries. This is not the case, however; M. Klaproth having set out for Kiachta with M. Helm, a botanist, in order to explore the frontiers of Chinese and Russian Tartary. At Kiachta M. Klaproth composed a Chinese and Japanese dictionary.

The university of Coimbra, in Portugal, has been enriched by a bequest of the library of Monsenhor Hasse, who lately died at Lisbon. The rare books and manuscripts of this rich library amount to nearly 12,000 volumes. Besides some Latin and Spanish works of the 15th century, it contains every thing, both in print and manuscript, upon the Portuguese legislation, as well as most of the scarce works upon Portuguese and Spanish literature.

An institution for the cure of the deaf and dumb has been established in Sweden. The number of these unfortunate beings is very great there: the dioceses of Upsal, Vexio, Calmar, Ikera, and Carlstadt, alone contain 287.

Basilius, a learned Greek physician at Constantinople, has published a collection of letters in order to form a pure modern Greek epistolary style. He has introduced into it several letters of Alexander Maurocordato, the Turkish minister who had so much influence in the affairs of the Ottoman cabinet between the years 1653 and 1699. These letters, which are the most interesting of the collection, are followed by some others of Nicolas Maurocordato, the son of the minister who was alternately prince of Wallachia and Moldavia. The work is also enriched with some notices upon learned Greeks; such as James Manas d'Argos, Gerasimus,

rasimus, and Dositheus patriarch of Jerusalem. The whole forms a volume of 340 pages in quarto.

M. D. Brewster, of Edinburgh, has invented a new angular and position micrometer, which serves also for a perspective micrometer; and a goniometer for measuring the angles of crystals. It also furnishes a method of ascertaining the apparent values of all angles seen in perspective, and also their real value, when one of the lines which contains the angle is either a vertical or horizontal line on a level with the eye. We believe this is the only instrument that can measure angles without the observer being at the angular point, and it has likewise all the properties of the common micrometer.

LECTURES.

Mr. Home's Lectures on the principal Operations of Surgery, given gratuitously to the Pupils of St. George's Hospital, commence in October, as usual.

Mr. Gunning, Surgeon to St. George's Hospital, will commence his Lectures on the Principle and Operations of Surgery on Monday, the 5th of October, at Eight o'Clock in the Evening, at his house, No. 45, Conduit-street, Hanover-square.

LIST OF PATENTS FOR NEW INVENTIONS.

To Gordon Howden, of Oxford-street, in the county of Middlesex, sadler; for a girth pannel, which most effectually prevents the saddle from getting forward upon any description of horses, however much nature may in the shape of the animal work against it. July 20.

To Charles Lucas Birch, of Great Queen-street, in the parish of St. Giles in the Fields, in the county of Middlesex, coachmaker; for improvements in the construction of the roofs and upper quarters of landaus, landaulets, barouche landaus, barouches, barouchets, curricles, and other carriages, the upper parts of which are made to fall down. July 21.

To John Philips, of East Stonehouse, in the county of Devon, stone-mason and sculptor; for his method or methods of constructing and removing offices, counting-houses, and other rooms, with desks, drawing boards, and other similar

milar conveniences; which method may also be applied in the constructing and removing bridges, cottages, sentry-boxes, and to other purposes or erections of a smaller or larger extent. July 28.

To Joseph Astley, of Borrowstounness, in that part of our united kingdom of Great Britain and Ireland called Scotland, chemist; for improvements in the manufacture of sal-ammoniac. July 28.

To Enoch Wood, of Burslem, in the county of Stafford, potter; for his method or contrivance of applying power for the purpose of raising water from a lower to a higher level. July 30.

To Robert Dickinson, of Long Acre, in the county of Middlesex, esq.; for certain improvements on or in machinery for improving turnpike and other roads, and for other purposes. August 1.

To Edward Coke Wilmot, of Birmingham, in the county of Warwick, gentleman; for his instrument for the purpose of warming beds, and which may be applied to various other purposes. August 10.

To Richard Rees, of Red Lion Passage, in the county of Middlesex, cutler; for certain improvements in trusses for persons afflicted with ruptures. August 25.

To Samuel Hill, of Whiteley Wood, in the county of York, saw-maker; for his method of making iron and steel backs for fixing upon and using with the blades of scythes and of straw and hay knives, whether the blades thereof be rolled, forged, cast, hammered, or otherwise manufactured. August 26.

To Ralph Dodd, of Exchange-alley, in the city of London, engineer; for his still or alembic with a refrigeratory worm or condenser, and a piston and rod, for the use of distillers, brewers, and other persons using the like machinery. September 8.

To James Day, of Church-lane, Whitechapel, in the county of Middlesex, merchant; for his method of making and compounding a certain liquid composition called Danzic or Dantzic spruce, or Danzig or Dantzic black beer. September 9.

METEORO-

METEOROLOGICAL TABLE,
BY MR. CAREY, OF THE STRAND,
For September 1807.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Thermometer. Noon.	Thermometer. 11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Aug. 27	63°	79°	70°	29·92	50	Fair
28	66	73	60	·78	51	Fair and windy
29	63	68	59	·91	32	Showery
30	61	67	54	·95	52	Fair
31	54	62	55	30·09	25	Cloudy
Sept. 1	56	69	53	·20	54	Fair
2	54	69	55	·19	60	Fair
3	54	68	56	·04	65	Fair
4	61	72	61	29·90	32	Fair
5	63	68	60	·68	42	Fair, with wind
6	54	63	46	·45	45	Fair, with wind
7	47	62	45	·76	57	Fair
8	44	60	54	30·00	60	Fair
9	55	65	55	29·51	0	Rain
10	55	56	44	30·00	24	Cloudy
11	41	59	45	29·95	20	Cloudy
12	43	57	44	·79	33	Fair
13	42	54	41	·97	35	Fair
14	40	57	45	·96	30	Fair
15	45	54	49	·99	45	Fair
16	45	56	44	·94	40	Fair
17	42	54	40	·91	42	Fair
18	38	57	45	·84	51	Fair
19	44	59	44	·98	49	Fair
20	38	59	52	30·21	40	Cloudy
21	50	59	55	29·99	15	Cloudy
22	52	58	56	·75	0	Rain
23	56	60	55	·55	0	Rain
24	55	65	53	·51	24	Cloudy
25	56	59	50	·39	10	Showery
26	47	63	49	·67	45	Fair

N. B. The Barometer's height is taken at one o'clock.

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END OF THE TWENTY-EIGHTH VOLUME.

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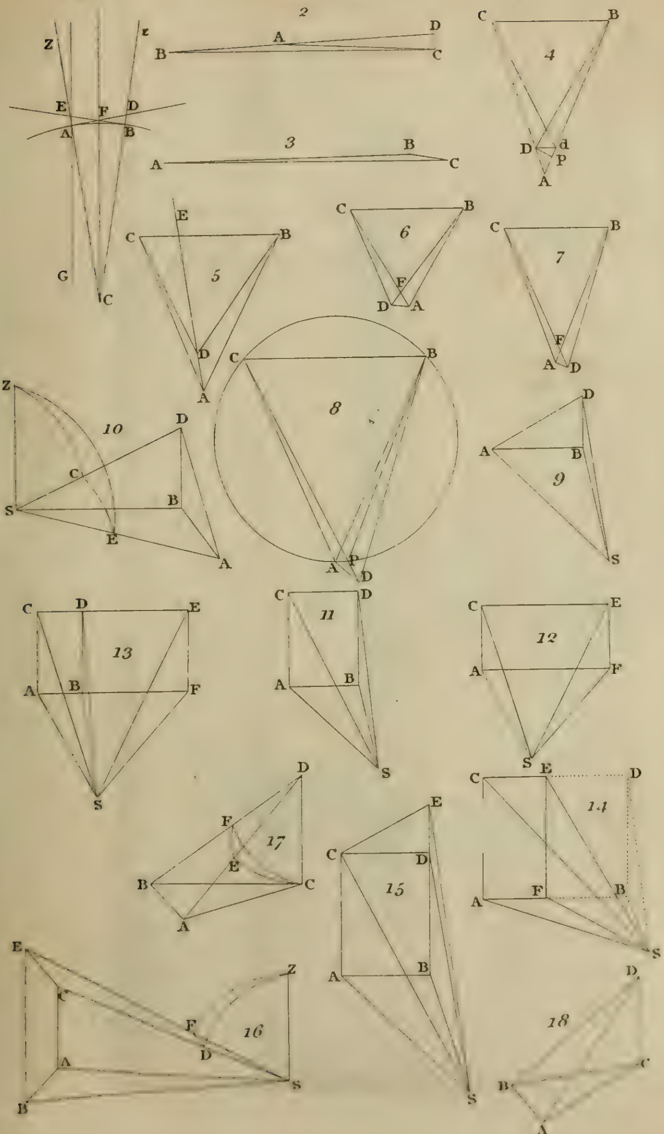
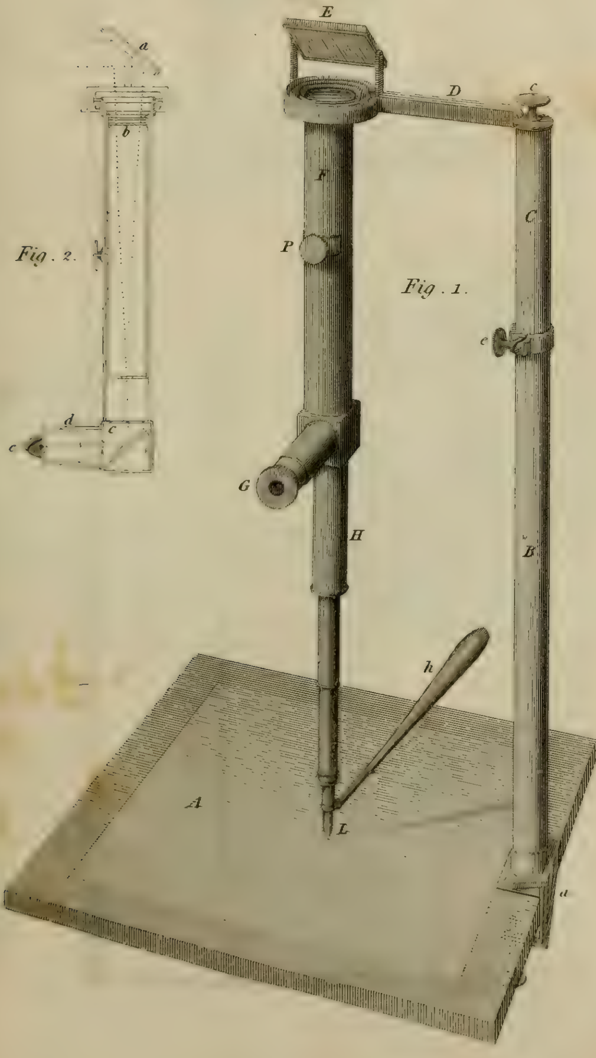
Goniometry.

Fig. 1.



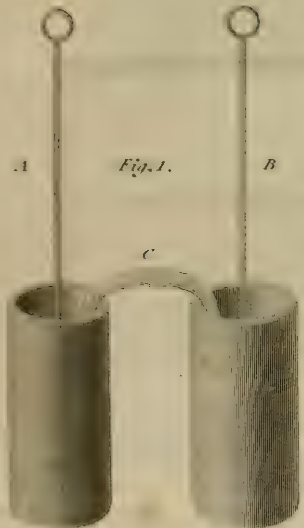
OPTIGRAPH.

By Thos Jones of Mount Street.



S. Porter sc.

M.r Davy's Galvanic Experiments.



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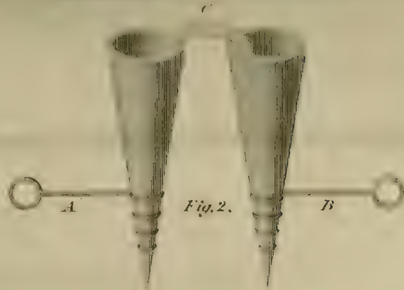

Fig. 2.

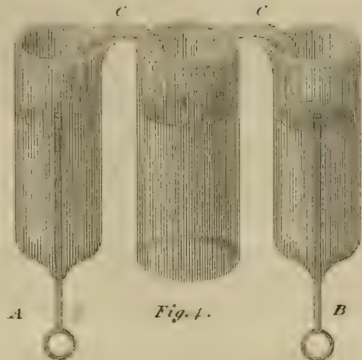

Fig. 4.

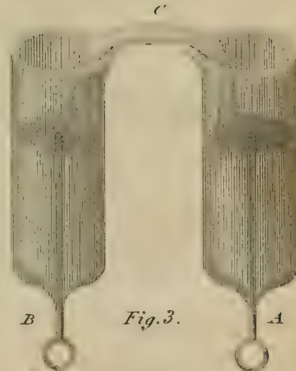

Fig. 3.

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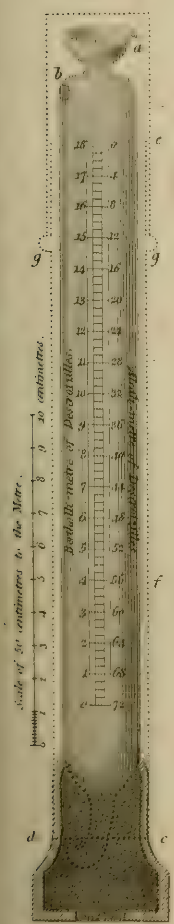
Decomposition of Light.

Fig. 3.



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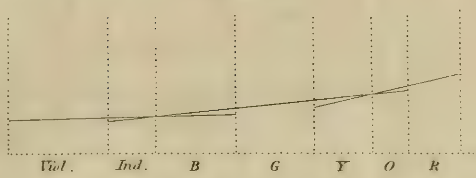
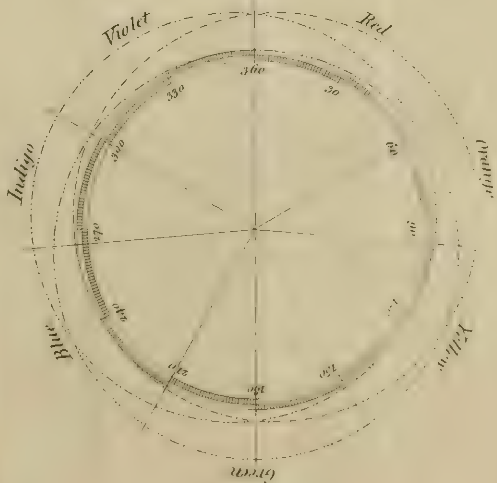


Fig. 2.



S. Porter sc.

Phil. Mag. Vol. XXVIII. Pl. V. p. 142.

by which they are expressed.
of the Royal Institution.
ohn Farey.) See Phil. Mag. XXVII. p. 195.

Ratios or Fractions.	Common Logarithms and Reciprocals.
2.922.977 / 2.923.003 &c. 42 +	·9999961,4666 / 38,5334
450.283 / 450.359 &c. 12 +	·9999266,501 / 733,499
0,000,000 / [illegible] &c. 9 +	·9997260,892

Fi

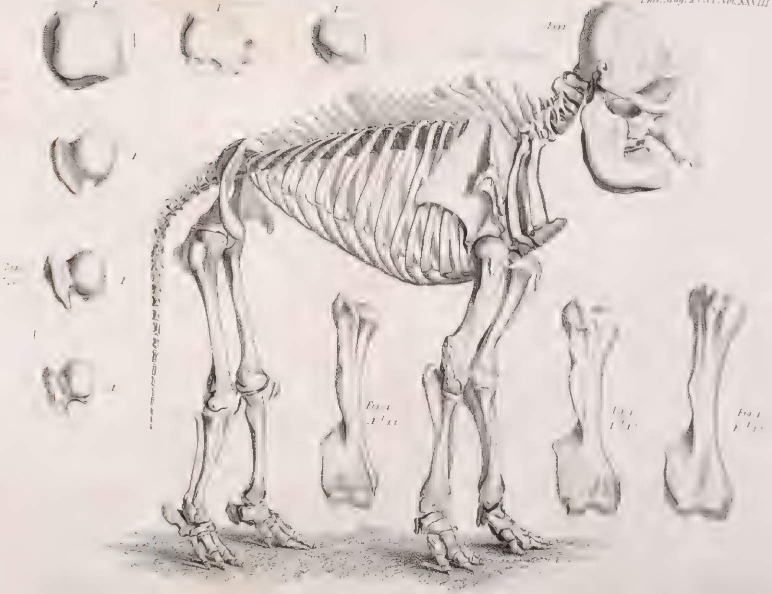
A Table of the smaller Musical Intervals, shewing the Symbols by which they are expressed in the Second, Boyce and Callcott Manuscripts in the Library of the Royal Institution; their Names and values in four different modes of Notation by Mr. John Farey; see Phil. Mag. XXVII p. 135.

Symbols	Names of Intervals	New Notation Σ, f and m	Indices of Powers of Primes	Ratios or Vibrations	Common Logarithms
m	Minute	m	− 161 84 12	1,922,077/1,922,103 &c 12+	·9999961,4666 38,5334
f	Lesser Fraction	f	− 54 37 − 2	450,283/450,359 &c 12+	·9999266,501 733,499
d	Medius Fraction	Σ − 3f + m	16 − 35 17	50,000,000/50,013,345 &c 9+	·9997260,892 2739,108
F	Greater Fraction	Σ − f + m	− 92 49 13	4,340,052/4,351,760 &c 21+	·9995793,8941 [illegible]
Σ	Schisma	Σ	15 − 8 − 1	32,768/32,805	·9995098,9286 [illegible]
r	Minor Residual	2Σ + f	− 24 21 − 1	[illegible]	·9989464,3585 10535,6415
ɩ	Medius Residual	3Σ + f	− 9 13 − 5	15913/16000 &c	·9984563,2871 15436,7129
ſc	Semi-Comma, Major of Rameau	4Σ + f	6 5 − 6	15,552/15,625	·9979662,2157 20337,7843
	Semi-Comma, Maxime of Ditto	5Σ + f	21 − 3 − 7	2,097,152/2,109,375	·9974761,1443 25238,8557
R	Greater Residual	5Σ + 2f	− 33 34 − 9	16,677,181/16,777,216 &c 9+	·9974027,645 [illegible]
R	Major Residual	6Σ − f + m	− 17 − 1 8	390,625/391,220	·9971288,5371 28711,4629
є	Minor Comma	10Σ + m	− 11 4 2	2,025/2,048	·9950950,7525 49049,2475
φ	Prisma	10Σ + f + m	− 65 41	36,472,996/36,611,488 &c 11+	·9950217,253 49782,747
c	Major Comma	11Σ + m	4 − 4 1	80/81	·9946049,6811 53950,3189
d	Diaschisma and Sum of IIIrd and V. temperaments	12Σ + m	19 − 12	524,288/531,441	·9941148,6097 58851,3903
	Dieze Minimum	14Σ + f + m	− 5 9 − 4	19,683/20,000	·9930612,9682 69387,0318
π	Hyperoche	15Σ + f + m	10 1 − 5	3,072/3,125	·9925711,8968 [illegible]
	Comma and Half	17Σ − f + 2m	− 13 − 5 9	1591/1990 &c	·9917338,2182 82661,7818
ε	Enharmonic Diesis	21Σ + 2m	− 7 3	125/128	·9897000,4336 [illegible]
ʃ	Semitone Subminimus	25Σ + f + 2m	− 1 5 − 3	243/250	·9876662,6493 123337,3507
δ	Chromatic Diesis	26Σ + f + 2m	14 − 3 − 4	16,384/16,875	·9871761,5779 128238,4221
ʃ	Semitone Minimum	32Σ + 3m	− 3 − 4 4	625/648	·9843050,1147 156949,8853
ʃ	Semitone Minor	36Σ + f + 3m	3 1 − 2	24/25	·98[illegible] 177287,6696
L	Limma	46Σ + f + 4m	− 8 5	243/256	·9773663,0829 226336,9171
S	Semitone Medius	47Σ + f + 4m	7 − 3 − 1	128/135	·9768762,0115 231237,9885
S	Semitone Major (H)	57Σ + f + 5m	− 4 1 1	15/16	·9719712,7640 280287,2360
P	Apotome	58Σ + f + 5m	11 − 7	2,048/2,187	·9714811,693 285188,307
S	Semitone Maximum	68Σ + f + 6m	− 3 2	25/27	·9665762,4451 334237,5549
4ε	Sum of III and 6th Tempts.	84Σ + 8m	− 28 12	244,140,625/268,435,456	·9588001,7344 411998,2656
t	Tone Minor (L)	93Σ + 2f + 8m	− 1 2 − 1	9/10	·9542425,0944 457574,9056
3ʃ	Sum of 3rd and VI Tempts.	96Σ + 9m	− 9 − 12 12	244,140,625/272,097,792	·9529150,3441 470849,6559
T	Tone Major (G)	104Σ + 2f + 9m	3 − 2	8/9	·9488474,7755 511525,2245
T	Tone Maximum	115Σ + 2f + 10m	7 − 6 1	640/729	·9434524,4567 565475,5433
3rd	Minor Third	141Σ + 3f + 14m	− 1 − 1 1	5/6	·9208187,5395 791812,4605
III	Major Third	197Σ + 4f + 17m	2 − 1	[illegible]	[illegible] 969100,1301
4th	Fourth	251Σ + 5f + 22m	− 2 1	[illegible]	[illegible]

ELEPHANTS. Pl. 2.

Engraved by S. Porter.

ELEPHANTS. Pl.



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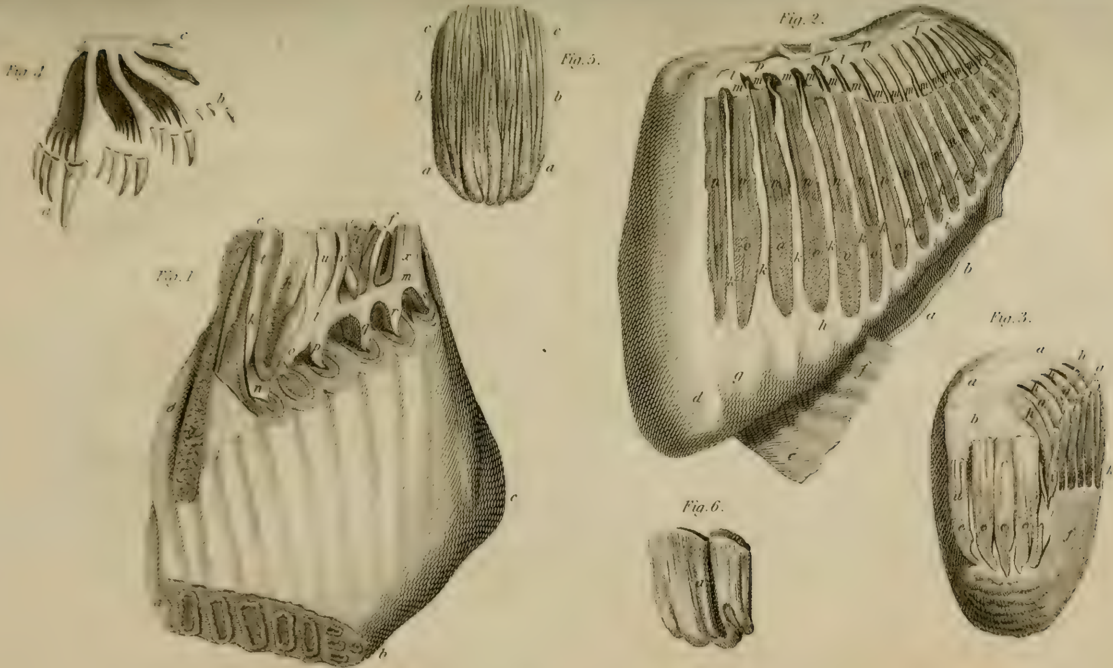

ELEPHANTS. PL. III.

J. Foster